EXPECTANCY IN APPARENT VISUAL MOVEMENT

BY E. E. JONES AND J. S. BRUNER
Harvard University

I. *Introduction* (p. 157). II. *Method* (p. 158). III. *Experiment I: procedure and results* (pp. 157–158). IV. *Experiment II: procedure and results* (pp. 159–161). V. *Experiment III: procedure and results* (pp. 161–163). VI. *Experiment IV: procedure and results* (pp. 163–165). VII. *Discussion* (p. 165). *References* (p. 165).

I. INTRODUCTION

The person responding to stroboscopic movement in the experimental room is familiar with a plethora of moving objects in a great variety of meaningful contexts before he ever comes to the experiment. What difference does this make? Out of familiarity comes a matrix of expectancies ordering and interrelating the cues to apparent movement. The perception of movement and its various attributes may be conditioned by these previously established perceptual expectancies. Put simply, the organism learns to expect movement in some contexts more than in others, and learns to expect certain *kinds* of movement from some objects and not from others.

The present series of experiments seeks to demonstrate some of the ways in which movement-related expectancies condition the perception of movement in a typical stroboscopic experiment. Expectancies were aroused by manipulating the nature of the objects in motion and the stimulus context in which movement occurred. The first two experiments deal with comparisons of the properties of apparent movement in 'meaningful' and 'meaningless' contexts. The second two are more concerned with the promotion of different patterns of movement when spatio-temporal factors are held constant, but the stimulus context and consequently the psychological meaning of movement is altered.

The importance of conditions related to past experience has been recognized by many students of movement phenomena, but most research on apparent movement has reflected an interest in the defining parameters of movement such as the interdependent spatial, intensity, and temporal factors. Wertheimer's early remarks suggesting the temporal intervals appropriate for simultaneity, optimal movement and succession, and the four laws of parametric interrelationship propounded by Korte, have been modified and extended by later research (see Graham in Stevens, 1951). Willey (1936) has demonstrated consistent individual differences in the resolution of ambiguous movement patterns. Duncker's (see Ellis, 1938*a*) exciting monograph on 'induced movement' opened a broad area concerning the relation between a moving object and its framework. But, by and large, research and theory in this area have been directed toward analysis of the *necessary and sufficient* conditions for producing stroboscopic movement. While such research must come first, it is also appropriate to extend the analysis of movement by attending to more cognitive determinants of the movement experience. It does not suffice merely to hold these constant—'meaning' held constant is indicative of the fact that we choose to overlook a large source of variance.

II. Method

In the experiments to be described below, eighteen Harvard students were used as subjects in Exps. I and II, while seventeen of these served in Exps. III and IV. They went through the experiments individually and in one sitting. The stimuli for apparent movement were projected upon a white cardboard screen held in a frame 22 in. high and 29 in. wide. The subjects sat at a slight angle to the screen, approximately 8 ft. from it. Slightly to the left of the subjects, directly facing the screen and $8\frac{1}{2}$ ft. from it, was a table on which were placed three Besler OA episcopes in such a way that the light projected by each covered an identical area coinciding with the limits of the white screen.

The three episcopes were wired to a timer regulating the exposure duration for each projector as well as the interval between exposures. The timer consisted of a Bodine Synchronous Motor driving a rotating shaft on which were mounted three pairs of circular steel disks with variable notches in their circumference activating micro-switches. The width of the notch in a given disk (wired to a given episcope) determined the exposure time for the episcope. The position of the variable notch on the disk determined the time interval elapsing between exposure of any two episcopes. The experimenter operated a simple control panel of three noiseless switches that opened or closed the circuit between the timer disks and each episcope. Movement could be produced by either two or three successive exposures depending on how many circuits were open.

In each episcope was placed a stimulus card on which pictorial material was drawn. The pictures used in the present experiment were all drawn in indian ink on white cards superimposed on larger black cardboard. The projected size of the white surface containing the stimulus pictures was, as we have said, coincident with the size of the white screen so that subjects were not distracted by nearby contours of the room, which was completely dark except for the episcopes in exposure.

Under these conditions, movement was quite easily created by the successive exposure of each episcope. The range of optimal movement was wide for any given subject. In the majority of the situations, movement could be maintained from as close to simultaneity as 0·1 sec. interval separation to as close to succession as 0·4 sec.

III. Experiment I: procedure and results

In the first experiment subjects were shown the sequence of cards presented in Fig. 1. In the successive exposure of the stimulus cards the 'stick-man' moved from the left side in the first phase to the right side in the third phase, changing places with the nonsense object which moved an equal distance in the opposite direction. Each subject was shown the picture sequence for one trial, asked to report his perception, given four more trials, and asked for a second report. Reports were spontaneous descriptions; no directive instructions were given. On each trial, the stimulus cards were separated by an interval of 0·15 sec., the exposure time being 0·20 sec. for each episcope. If a subject saw both objects in motion, he was asked to compare the objects with regard to speed of movement and relative distance travelled.

The results need no statistical elaboration. After the first trial, fifteen of the eighteen subjects reported seeing the man as moving faster than the nonsense object, and sixteen of the eighteen reported that the man covered a greater distance in his movement. Both results are at the 0·01 level, computed by the Dixon & Mood (1946) sign test. One of the

most striking findings was that on the first trial, six subjects (or one-third of the cases) saw only the man as moving and in one case the nonsense object was not even seen.

On the fifth trial this relationship no longer obtains. Eight of the subjects saw the man and the object moving at an equal speed, and eight of the remaining ten saw the nonsense object as moving faster. This same shift occurs with respect to the perception of distance travelled. We may conclude that the results are not simply a function of constant position factors—that left-to-right movement is somehow more compelling than right-to-left movement. A more reasonable conclusion is that a man-in-motion has a higher subjective probability value than a thing-in-motion. This expectancy operates to accentuate the perceived velocity and distance of the man's movement relative to that of the nonsense object on the first trial when the actual spatial-temporal features are still equivocal, and when the objects are not yet clearly localized within a stable framework. When the framework is established, there appears to be a tendency toward reversal. We do not know how to account for this effect save to suggest that Köhler-Wallach (1944) satiation may be operative in apparent movement.

Fig. 1. Stick-man and nonsense objects.

IV. Experiment II: procedure and results

In the second experiment we turn to another way in which a meaningful relationship between subjective expectancy and context may accentuate apparent movement. Optimal stroboscopic movement can only be maintained within certain limits of spatial and temporal separation between stimuli. One might expect these limits to be broader when the context in which the movement occurs is one in which motion has been frequently experienced. In other words, movement in an often experienced context would be expected to maintain itself more effectively under suboptimal conditions than movement without such experiential support.

Fig. 2 shows the sequence of cards used in the present experiment.

All of the eighteen subjects were initially exposed to thirty trials of both the Control and Experimental Series, ten trials each at separation intervals of 0·15, 0·20 and 0·25 sec. Then each subject was further exposed to fifteen successive presentations of the control series (oblong-over-arc stimulus) and fifteen successive presentations of the experimental series (car-over-bridge stimulus). The exposure for each card in the two series was 0·20 sec., with an interval between cards of 0·15 sec. Half of the subjects received the experimental series first and half of the subjects received the control series first. On four randomly interspersed trials in each series, the middle stage of movement was omitted by cutting the current to the centre episcope. Thus the objective stimulus consisted merely of a car at one end of the bridge and then a car at the other end, or the nonsense version of the same two exposures.

In the control series of fifteen trials, two-phase exposures occurred on trials 4, 7, 10 and 11. In the experimental series, two-phase exposures occurred on trials 5, 8, 11 and 12.

The subjects were told: 'I'm going to show you the same pictures in a series of fifteen consecutive trials. I want you to look at the total series to judge whether there seems to be any progressive trend in goodness of movement from trial 1 to trial 15. In other words, does it seem to be getting consistently faster, or slower, or going farther or less far, or seem smoother or jerkier? I want you to report specific fluctuations from trial to trial—as you see them—on any or all of these characteristics.'

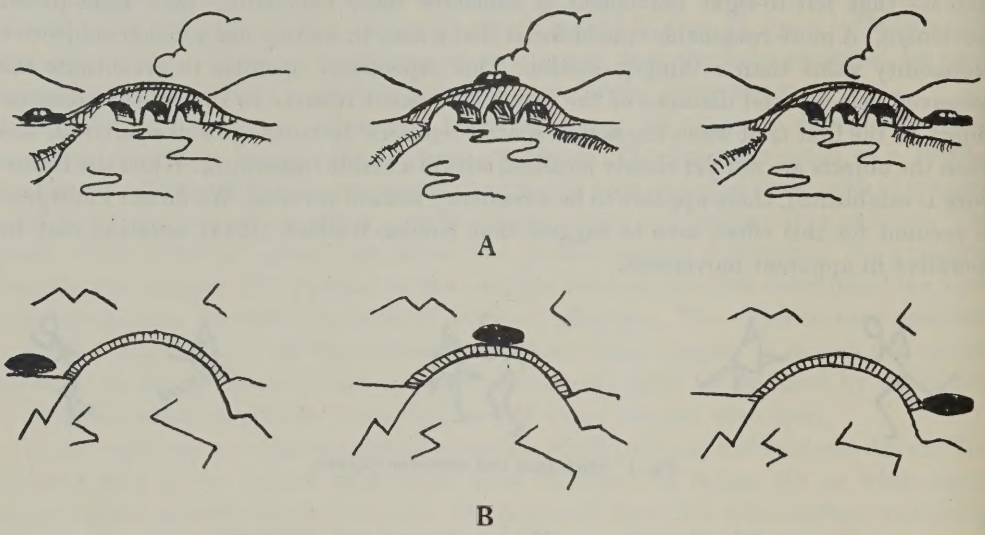


Fig. 2. A, car-over-bridge context; B, oblong-over-arc context

Table 1. *Responses to two-exposure trials for experimental (auto) and control (oblong) series*

	Control	Experimental
No change in character of movement	36	57
Change in character of movement		
(*a*) Unevenness noted 15	36	5 15
(*b*) Middle image missing 21		10
Total number of responses*	72	72

* This total represents four responses for each of the eighteen subjects.

On the basis of differences in strength of movement-expectancy, we would predict more disruption of movement on critical two-phase trials under nonsense conditions. That is, though the expectation of three-phase movement is reinforced in the experimental situation for both card sequences, movement in the more meaningful context will be *relatively* less affected when the mid-phase is absent, since the expectancy in the latter case is well buttressed by previous confirmations outside of the experimental situation. Table 1 classifies the spontaneous response to the trials in which the middle exposure was omitted.

Without the benefit of statistical test[1] these differences seem rather impressive by inspection: 79% of the observations under the experimental condition were reports of no change, whereas only 50% of the control reports were 'no change' responses; only 14% of the responses under experimental conditions indicated perception of the missing middle image, whereas 29% of the control responses did so.

[1] Inadvertently the original data for this experiment were destroyed. The combined data in Table 1 do not permit the employment of such usual tests of contingency as χ^2.

To the extent that these differences are reliable, we conclude that the meaningful context more effectively mediates the expectancy than does the nonsense situation. The more the stimulus context of movement is symbolically appropriate, the less susceptible will the experience of movement be to disruption by a change in physical conditions away from the optimal state—such as a reduction in the number of exposures in a given movement space.

V. EXPERIMENT III: PROCEDURE AND RESULTS

In the present experiment our object was to study the patterning of movement as a function of the intrinsic characteristics of the field. Fig. 3 contains the stimuli used. In one version of the three-phase movement, the ball on the inclined plane is seen to *skid*—a movement mediated by the constancy of position of the inset arc. In the other version, the ball is seen to *roll* down the inclined plane—the arc changing position appropriately

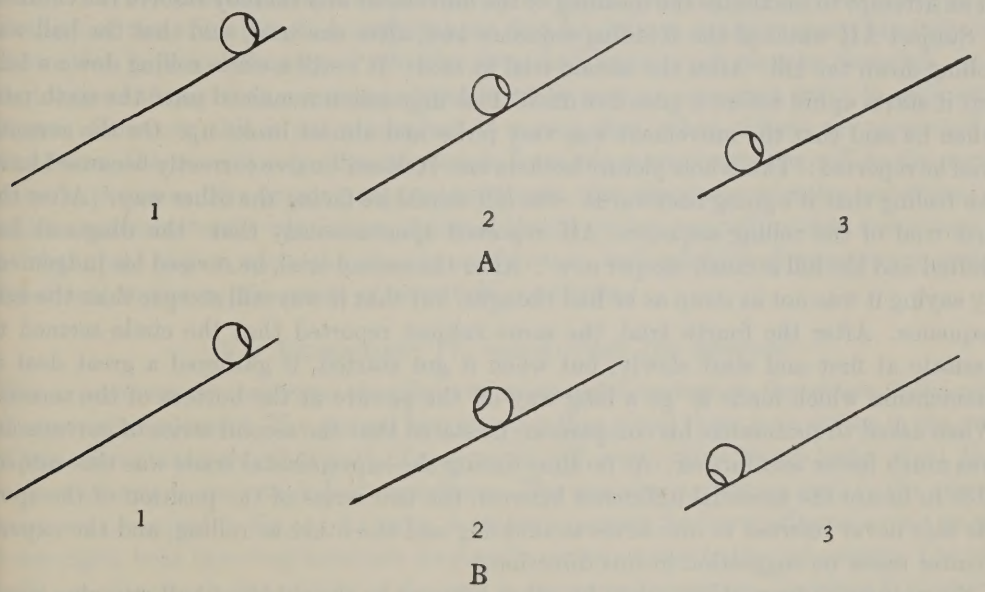


Fig. 3. A, 'skidding' ball context; B, 'rolling' ball context.

in the three exposures. Movement characteristics should be appropriate to each context —a skidding context and a rolling context. Thus, apparent speed and distance traversed should be greater in the second instance.

Each of seventeen subjects was shown the 'skidding' ball (Fig. 3A) for ten trials at an exposure time of 0·20 sec. and a separation-interval of 0·15 sec. They were then shown the 'rolling' ball (Fig. 3B) for ten trials and asked to compare the two sets of exposures. Direct questions were not asked for fear that they be leading questions, but the subjects were asked to describe what they saw, and an attempt was made to classify their spontaneous responses.

Eleven of the seventeen subjects saw the ball in Fig. 3A as skidding, none of them saw it consistently as rolling down the plane, and the remaining six subjects saw it in a reverse roll or in some other fashion. Where the ball in Fig. 3B was concerned, twelve subjects

saw it as rolling, none as skidding, and the five others gave responses that were not classifiable. The test of our hypothesis comes in the comparison of the two forms of movement. Fifteen of the seventeen subjects saw the 'rolling' ball sequence as faster, thirteen of the seventeen as covering a greater distance (χ^2 yielding values in both cases at less than the 0·05 level). Quite evidently, the 'rolling' ball was seen to have more velocity and more momentum.

Qualitative distinctions between the two movement experiences were clear. The difference between the two series was described specifically as a difference between skidding and rolling circles by eleven of the subjects, and in all cases the two movement sequences resulted in qualitatively distinct response patterns. Some of the spontaneous comments of the subjects are presented below to indicate the variety of attempts to resolve the tension produced by the circle with the constant inset arc. Many of these attempts involve an active reorganization of objectively constant background features in an attempt to maximize the meaning of the movement and thereby resolve the conflict.

Subject AH watched the skidding sequence and, after one trial, said that the ball was rolling down the hill. After the second trial he said: 'It's still a circle rolling down a hill, but it starts uphill before it goes downhill.' This impression remained until the sixth trial when he said that the movement was very jerky and almost broke up. On the seventh trial he reported: 'This whole picture bothers me. It doesn't move correctly because I have the feeling that it's going backwards—the hill should be facing the other way.' After the first trial of the rolling sequence, AH reported spontaneously that 'the diagonal has shifted and the hill is much steeper now'. After the second trial, he revised his judgement by saying it was not as steep as he had thought, but that it was still steeper than the first sequence. After the fourth trial, the same subject reported that the circle seemed to hesitate at first and start slowly, but when it got started, it gathered a great deal of momentum which made it 'go a long way off the picture at the bottom of the screen'. When asked to summarize his comparison, he stated that the second series of movements was much faster and farther. At no time during the experimental trials was this subject able to locate the essential difference between the two series of the position of the spot. He also never referred to one series as skidding and the other as rolling, and the experimenter made no suggestion in this direction.[1]

Spontaneous observations given by other subjects to the skidding-ball stimulus were: 'The ball is pushed down, it skids to a stop.' 'I see a ball, but I can't tell whether it rolls up the hill or down.' 'The movement is very jerky and the ball stops a long way from the edge of the screen.' 'I'm surprised to see it [apparently the arc] reappear in the same position. You look for it in a different place and you don't see it.' 'It gives me a retarded sense and seems to skid to a stop.' 'The ball isn't rolling, but it's balanced on a string and somebody's pulling the string.' 'A ball is rolling down a wire but something is funny. I can't spot what that extra line is, but the whole thing seems wrong.'

It might be pointed out, as a final interpretative note, that this experiment sheds further light on the concept of *identity* in stroboscopic movement. Linke (see Neff, 1936) was the first to affirm that phenomenal identity of initial and terminal stimuli was the one essential determinant of apparent movement perception. But it was Ternus (see Ellis,

[1] The reports of AH argue against explaining the observed modification of perceived movement in too intellectualistic terms. At least in his one case, this response is not a logically considered comparison but an immediate and unreflective response to the total field.

1938*b*) who underlined the ambiguity of the identity concept by demonstrating the complexity of spatial identity in even a simple stimulus situation. Working with four equidistant points lying along a straight line, Ternus presented the first three points as the initial stimulus, and the last three points as the terminal stimulus (so that the two middle points were exposed both times and in the same position). Instead of seeing a single point move from the first to the fourth position, his observers perceived the movement of a line composed of three points. In the movement of such a figure, only homologous parts (parts playing the same role) exhibit phenomenal identity, since 'phenomenal identity is a characteristic not of piece-wise relationships, but of whole-phenomena' (Ellis, 1938*c*). Identity must be appropriate to the pattern of movement.

Orlanski (1940) addressed himself to a different kind of identity—the identity of stimulus form in successive exposures rather than spatial location. Progressively changing the 'identity' of his stimulus objects by varying formal dimension, he showed that the stability of movement (using a range index of stability) varied directly with the objective identity of the stimulus. It is unfortunate that he did not examine more carefully the distinction between identity of stimulus form and identity of stimulus function. It is tautological to say that a moving object must be seen as a moving unity in sequential stages. However, this is not equivalent to saying that, when objects are presented in succession, the more closely alike they are in form, the more compelling will be the movement experience. The present experiment shows that there are stimulus conditions in which movement under conditions of *formal* identity is *less* compelling than movement of a 'changing' object when the change conforms to well-established expectancy and is functional to the requirements of total field conditions.

VI. Experiment IV: procedures and results

This experiment demonstrates the effects of meaningful context on the option of the path of apparent movement. The general hypothesis is that where there is an option in the path of apparent movement, that path of movement will occur, other things being equal, that most conforms to an established expectancy. The stimulus sequences used in the experiment are presented in Fig. 4. In series A we see a man on the lower left and a man on the lower right, both throwing baseballs diagonally across a space to the top corners. The two balls are adjacent in the middle of the card in the second exposure. In series B there are two billiard players, both of whom are hitting billiard balls from the upper two corners of the pool table, diagonally across to the two lower corners. In the mid-phase of movement the balls are again seen to be adjacent. Series A was shown at a separation-interval of 0·20 sec. and an exposure time of 0·20 sec. per card. After several intervening experiments, series B was presented at the same interval and exposure. Both series were presented for ten trials.

After each trial of each series, the subject was asked to report all the impressions of movement he had received. If he insisted in centering his responses around the 'actors' in either series, the experimenters directed his attention by more specific questions to the course or movement taken by the balls. In all but a few cases, the report situation was unstructured. Responses to each of the ten exposures were recorded for each series. If the situation was not understood at all (as was frequently the case with the pool players), the initial stimulus card was taken from the episcope and shown to the subject.

These particular sequences were chosen in an attempt to overlay a basic pattern of

movement ambiguity, described by Ternus (see Ellis 1938*d*), with symbolic contexts which would predispose the subject to choose a specific alternative. Ternus had found that the apparent movement of dots placed as ours were could be seen either as 'crossing over' or as 'cannoning' or 'bouncing' off each other. In designing the stimuli, it seemed to us more probable that two thrown baseballs would not hit each other in mid-air and cannon

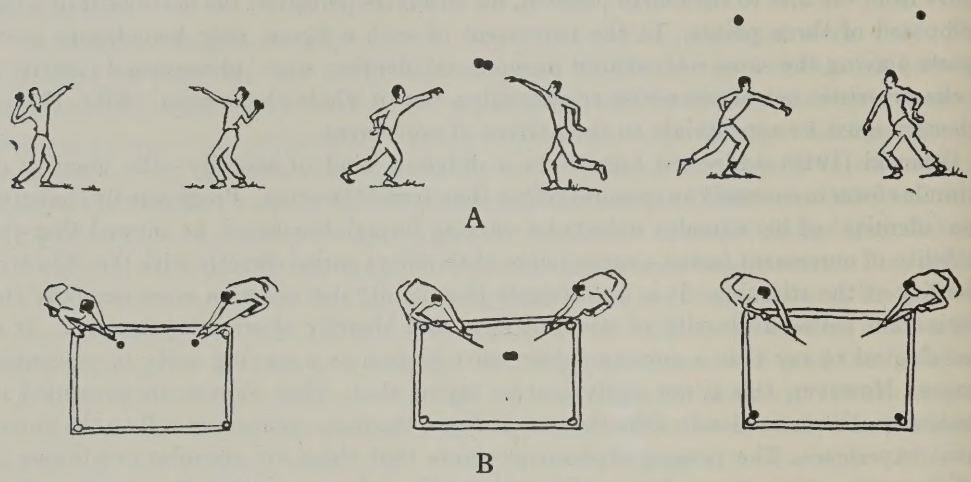


Fig. 4. A, baseball context; B, billiards context.

Table 2.† *Responses to ambiguous movement patterns in two contexts*

	Cross over	Bounce	Total	
Baseball contest	10	5	15	$\chi^2 = 1{\cdot}6667^*$ $P = 0{\cdot}1998$
Billiard context	4	11	15	$\chi^2 = 3{\cdot}2667^*$ $P = 0{\cdot}0753$

† Table 2 contains a comparison of *each* context against 50:50 null hypothesis. Direct comparison of the two contexts with each other is not possible because the same subjects were used in each context, thus violating the requirement of independence necessary for direct comparison by the χ^2 technique. Other, less direct techniques, could not be applied because of the inadvertent loss of the raw data previously mentioned.

off at an angle. On the other hand, it seemed very probable that two pool balls might collide and be deflected into opposite corners.

The results tend to support the prediction but not conclusively. In the majority of reports, the pre-existing expectancy of the subject promoted the predicted option for each sequence. Disregarding the 'ambiguous' and 'no movement' reports of two subjects, we can construct a contingency table (Table 2). The association between context and response option (pooled probability) is nearly significant ($P = 0{\cdot}076$). Thus the same basic 'carriers' of the movement experience may be seen in two qualitatively distinct patterns when different expectancies are involved.

VII. Discussion

This paper has attempted to demonstrate the operation of a variable which conditions the perception of visual movement. The variable of our interest may be called variously stimulus *context* (the experimentally manipulated stimulus framework in which movement occurs), *expectancy* (the pre-established network of hypotheses about movement in the subject), or *meaning* (the relationship between stimulus context and subjective expectancy).

We have cited and briefly described four experiments in which the relationship of stimulus context to postulated general expectancies was shown to be an important source of variation. The evidence from these experiments supported the following hypotheses:

Experiment I. When two objects cover an identical distance objectively, the object which has more frequently been seen in motion will be seen initially as moving faster and farther.

Experiment II. The more probable and practised an apparent movement, the more adequately will the movement experience maintain itself under sub-optimal conditions.

Experiment III. Speed and extent of apparent movement are related to the qualitative form of movement promoted by the stimulus context.

Experiment IV. When the direction of movement is basically optional or ambiguous, there is a tendency for contextual cues to promote one option or another according to the expectancy aroused.

The experiments reported here are of the order of 'demonstration experiments'. It would be a further amplification of the problem to establish experimentally certain expectancies of movement in our subjects and to work with these as independent variables. Our object in reporting the present experiments has been to underline the importance of such cognitive variables as expectancy in the determination of speed, extent, path, and consistency of apparent movement. Research on the parameters of apparent movement perception has generally neglected consideration of the functional overtones in our apprehension of motion. The variables of time and intensity of exposure, interexposure interval, and spatial separation do not by any means exhaust the variance to be found in the movement experience.

REFERENCE

DIXON, W. J. & MOOD, A. M. (1946). The statistical sign test. *J. Amer. Statist. Ass.* XLI, 557–66.

ELLIS, W. D. (1938). *Source Book of Gestalt Psychology:* (*a*) pp. 161–72, (*b*) pp. 149–59, (*c*) p. 154, (*d*) pp. 149–160.

KÖHLER, W. & WALLACH, H. (1944). Figural after-effects. *Proc. Amer. Phil. Soc.* LXXXVIII, 269.

NEFF, W. S. (1936). A critical investigation of the visual apprehension of movement. *Amer. J. Psychol.* XLVIII, 1–42.

ORLANSKI, J. (1940). The effect of similarity and difference in form on apparent visual movement. *Arch. Psychol., N.Y.*, p. 246.

STEVENS, S. S. (ed.) (1951). *Handbook of Experimental Psychology.* New York: John Wiley; London: Chapman & Hall.

WILLEY, C. F. (1936). Directional variations of apparent movement. *J. Exp. Psychol.* XXIX, 655–73.

(*Manuscript received* 23 *July* 1953)

THE CHANGE OF MOVEMENT TIMING WITH AGE

By W. T. SINGLETON

Nuffield Research Unit into Problems of Ageing, The Psychological Laboratory, University of Cambridge

I. *Introduction* (pp. 166–167). II. *The four-choice experiment* (pp. 168–170). III. *The two-choice experiment* (pp. 170–171). IV. *Discussion* (pp. 171–172). *References* (p. 172).

I. Introduction

There appears to be a widespread belief that older people are slower than younger ones in the speed at which they execute movements, rather than in the speed at which they make decisions and initiate actions. The evidence for this opinion originates from the work of Miles (1931). Using hundreds of subjects on a variety of tests, he was able to show convincingly that, although decrements with age were always surprisingly small, they were more apparent in what he called 'motility measures' than in 'reaction tests'. He defined a motility measure as one including movement only, thus distinguishing it from a reaction measure, which 'included also the strictly cortical elements of comprehension, selection, and readiness for response plus a brief movement' (*op. cit.* p. 632). For reaction tests he used the standard reaction time procedures for both hand and foot, for motility measures he used handle-winding, and a test in which the subject had to move his hand from a key, take a pencil from one vertical hole, insert it in another and return the hand to the key. In recent years, results have accumulated which are not readily reconcilable with this classical view.

Szafran (1951) tested ten subjects in each age decade from twenties to fifties, on an aiming task. The subject sat in the centre of a wooden framework and moved a stylus from a central resting point to any one of a number of 14 in. targets, with 1 in. metal centres, distributed on three sides of him. The task was given under two conditions—with and without vision. When subjects could use direct vision, the time taken to move from the resting point to contact with the target increased slightly with age, but not significantly, and comparatively less than the various stationary components, such as time to initiate response. In this condition the subjects all moved directly to the centre of the target. Without the aid of vision, subjects tended to move the stylus generally on to the particular target and then spent a comparatively long period on target searching for the target centre. In this condition, the time to move from the resting position to the target showed a consistent though not significant decrease with age while other time components of the task increased considerably with age.

Leonard (1952) used a task in which the Display consisted of five lights arranged in a regular pentagon, and the Control was a similar arrangement of metal disks inset in a wooden panel with a sixth disk at the centre of the pentagon formed by the outer disks. The subject moved a stylus from the centre disk to the outer one corresponding to the particular light on the Display. Only one light was on at any time. When the stylus was returned to the centre after touching the indicated disk, the old stimulus light was extinguished and another one appeared. Thus, the task was a serial one involving sliding

the stylus to and fro between the centre disk and the outside ones, following unpredictable signals given on the Display. Using a group of young subjects (average age about 25), and a group of older subjects (average age about 65), he found that while all the time components of the task increased with age, there were indications that the speed decrement was greater for the times on the centre disk and on the outer disks, than it was for the transit times between disks. The disks were $1\frac{1}{4}$ in. in diameter, and the distance between the edge of the centre disk and the edge of any one of the outer disks was $1\frac{3}{4}$ in.

Both Leonard and Szafran were of the opinion that, although the results in the above experiments were not conclusive, they were consistent with the hypothesis that older people's performance differs from that of younger ones more in the time taken to make decisions than in the time taken to carry out actions.

The findings in all the experiments so far described are easier to reconcile if it is remembered that Miles's distinction between 'motility' and 'reaction' is very difficult to realize in a practical experimental situation. There is both psychological and physiological evidence to support the view that when a manual skill is being performed, a continuous stream of information is passing from the peripheral to the central parts of the nervous system. The concepts of a discrete stimulus—a physical change in the environment which produces a new pattern of sensation which then enters the central mechanisms, and, after a reaction time interval, is translated into muscular action—is a gross over-simplification. The time of an overt movement is very rarely a function of the relevant peripheral processes alone. A movement involves a continuous inflow of nerve impulses both through the proprioceptive system and usually through the optic system, since the eyes are focused on either the moving member or the destination or both. If the movement has some unfamiliar aspects, then the time it takes depends on the speed at which information can be received and further 'orders' retransmitted through the effector system. In other words, a motility measure is usually just as much an index of time taken by central processes as is a reaction measure.

The central processes acting in relation to a movement are essentially concerned with guiding the moving limb, that is, controlling its direction and velocity at any time. If a certain degree of guiding is imposed externally, then it is reasonable to postulate that there will be a corresponding decrease in the extent of central involvement during the movement. One way in which this can be achieved is to ask the subject to move a lever for which the possible directions and amplitudes of movements are mechanically fixed, rather than simply presenting him with starting and ending points and allowing him to find his own path between them.

The author measured the time for an 18 in. movement under two conditions:

(*a*) The subject had to move a stylus to and fro between two disks, each 1 in. in diameter. He was to move as quickly as possible for about half a minute and to take care not to miss touching a disk. The average time of a group of subjects for a single cycle of movement was about 1 sec.

(*b*) The subject grasped a knob at the end of a lever. The lever was constructed so that the knob could be moved only 18 in. in a straight line, with sprung stops at each end of the traverse. The subject was told to move as quickly as possible from one end to the other for about half a minute. The average time of a group of subjects for a single cycle of movement was about half a second.

Detailed results of this experiment, including changes with age, will be published later.

Here it is mentioned as confirmatory evidence that, when a movement is partly guided by external physical constraints, the maximum speed is very much higher than when more control has to be exercised by the subject.

The experiments already cited can now be reconsidered in the light of the following propositions:

(*a*) That the central involvement in action decreases as the accuracy of action adequate for the task decreases.

(*b*) That differences in performance between older and younger people become more marked as the central control required during the performance increases.

All the tasks used by Miles as motility measures were such that some changes of direction of movement were included in the time intervals measured. It may be that it was this characteristic which produced the decrement with age which he found. In the Szafran experiments, there was some decrement with age when the movement was directed towards the small target centre, but no decrement when conditions were altered so that the subject could only hope to move generally on to the large target area. In the Leonard experiment, the relative dimensions of the targets and their distance apart were such that only a very low degree of movement accuracy was sufficient to achieve the desired result. In this case the change with age in movement time between disks was small compared with that found in time spent on the disks where changes in the direction of movement were necessary.

To obtain further evidence in support of this point of view, the following experiments were carried out. The apparatus was developed from that first designed by Leonard (1952); the data reported were obtained from details of performance recorded on an electronic-digital machine—the S.E.T.A.R. (Welford, 1952).

II. The four-choice experiment

Apparatus. The Display consisted of four lights arranged in a square, with one diagonal vertical. The Control was a lever, which could move along four directions from a centre point as shown by the shaded area in Fig. 1. Each light signal indicated a particular direction of movement. During a trial, a stimulus light could be extinguished by moving the lever to the appropriate E; when the lever was then returned to C a new stimulus light would appear. The Control and Display were alined so that a light could only be put out by moving in the direction opposite to that where the light was on, e.g. a light on at the left meant move to the right, a light on at the top meant move towards the body and so on. In this way the task was made more difficult than it would have been had a directly alined Display and Control been used (Singleton, 1953).

For the purpose of this paper it is sufficient to note that the task was to move a lever C to E, E to C, C to E, and so on in response to light signals. The lever was sprung to facilitate stopping and changing direction at C. A movement C to E required a knob displacement of about 4 in.

Method. Ten male subjects in each age decade were used. They were selected at random from acquaintances of the experimenter, visitors to the Laboratory, University employees, etc., with the one criterion that in each age decade there should be five subjects of approximately University or professional status, and five at about the average educational level of the wage-earning community. Thus in the twenties five subjects were

students and five were naval ratings. Each subject was given six trials on the task; a trial consisted of sixty-four correct responses.

Results. For each correct response three time measures were recorded: the time spent at C, that is the time between the appearance of the stimulus and the initiation of the response; the time spent in moving from C to E; and the time spent between

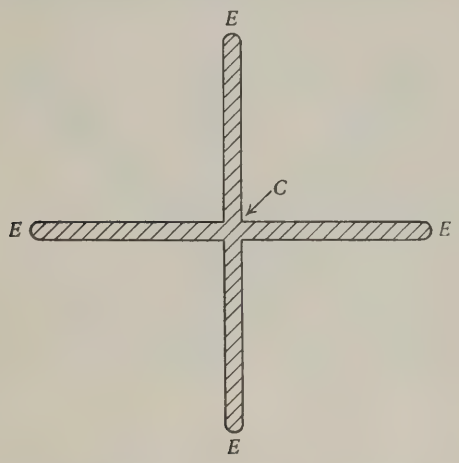


Fig. 1. The shaded area represents the possible directions of movement in the Four-Choice Experiment.

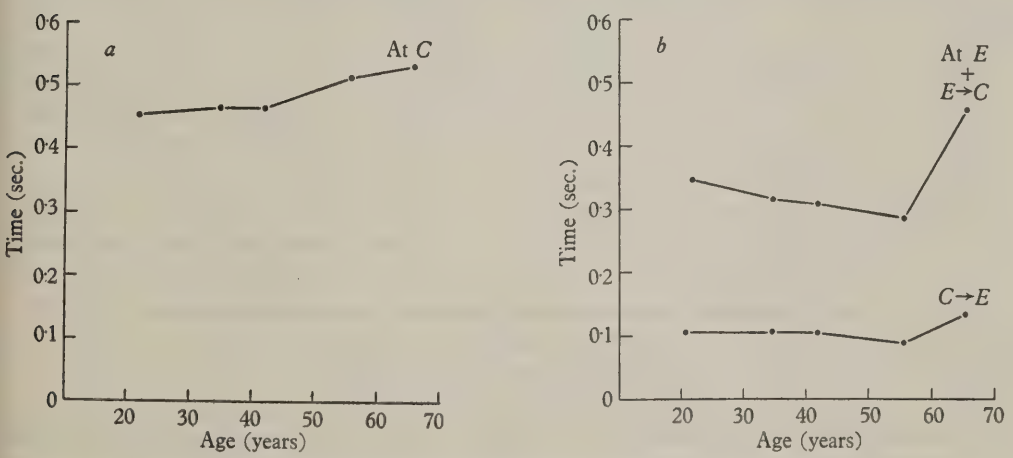


Fig. 2. Component times as function of age in the Four-Choice Experiment.
(*a*) Stationary times; (*b*) moving times.

arriving at E and arriving back at C. When a subject made a mistake (that is, started from C towards an E which would not extinguish the light), his centre time for that response was omitted. The distribution of errors with age is not easy to analyse, but there is certainly no marked linear trend of either increase or decrease from 'twenties' to 'sixties'.

All three measures taken during the sixth trial are shown as means for subjects in each age decade in Fig. 2*a*, *b*. The time (at C) increases linearly with age, the time (C to E) remains reasonably constant with age. The time (at E plus E to C) appears to

decrease from 'twenties' to 'fifties', and shows an abrupt increase for 'sixties'. This unusual function is the subject of further experiments not immediately relevant to the present issue, which will be described in a later paper. It may be noted, however, that it is consistent with the results of Szafran (1951), who found a small but regular decrease in movement time from 'twenties' to 'fifties' and with Leonard (1952), who found an increase in movement time when a group of 'twenties' was compared with a group of 'sixties'.

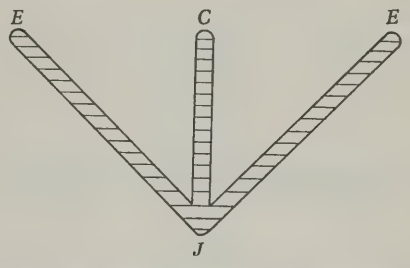


Fig. 3. The shaded area represents the possible directions of movement in the Two-Choice Experiment.

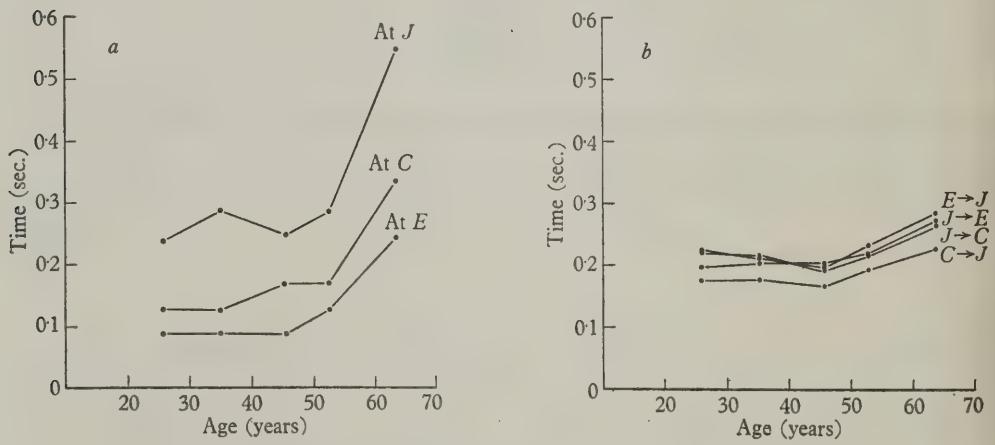


Fig. 4. Component times as functions of age in the Two-Choice Experiment.
(*a*) Stationary times; (*b*) moving times.

III. The two-choice experiment

Apparatus. The Display consisted of two lights arranged horizontally about 9 in. apart. The Control was a lever which could move along the directions shown by the shaded area in Fig. 3. During a trial the stimulus light could be extinguished by moving the lever to the appropriate *E*, when the lever was returned to *C* via *J* a new stimulus light would appear. The Display and Control were directly alined so that, for example, a light on the left meant move from *C* to the left *E*. The lever was lightly sprung in the general direction *JC*. A movement *C* to *J* required a knob displacement of about 4 in., from *J* to *E* about 6 in.

Method. Forty-eight male subjects were used, fairly evenly distributed over the age range from 20 to 70 years. All were employees of a Lancashire cotton mill. Each subject was given one unscored practice trial followed by two experimental trials on the task. A trial consisted of forty-eight correct responses.

Results. For each correct response eight time measures were recorded. The time spent at C, moving from C to J, at J, moving from J to E, at E, moving from E to J, at J, and moving from J to C. Almost all the errors made were manipulatory ones at J; such error times at J were omitted in calculating the means. There was no marked error trend with age. The data of Fig. 4 were extracted from records of the final trial.

The changes with age in the four moving times are all of the same form. The stationary times against age curves are all three of the same form but, as a class, appear to be different from the moving times. The 'at J' component is the sum of 'J going out' and 'J returning'. These latter do not appear to differ from each other as functions of age. It will be noted that the curve 'at E plus E to C' of Fig. 2 is of the same form as the stationary times in Fig. 4, which suggests that the 'at E' component is controlling this function at least at the upper end of the age scale. The stationary time 'at C' in Fig. 2 is not the same shape as the stationary times in Fig. 4, presumably because the former is determined by time necessary to receive new information rather than time to change direction. This may be one case of a phenomenon which merits further investigation. The central processes associated with changing a movement direction appear to overlap in time with perceptual processes determining the next direction of movement.

IV. Discussion

The manipulative skills performed in ordinary life are usually such that information concerning what must be done next becomes available while other movement is being executed, and some time before it is necessary to take appropriate action. This introduces an ambiguity in the results obtained by recording the time components of such performance. It is not known whether a particular time interval is a function of the 'tactical' action of moving a limb from one point to another, or of some more 'strategic' process concerned with the planning of future action. The tasks described above were essentially artificial, in that information concerning the next action only became available when the previous action was complete, and the degrees of freedom in carrying out movements were quite strictly determined by the apparatus.

Within these limitations, the results of the two experiments suggest that the performance of older men on simple, but somewhat unfamiliar perceptual motor skills, tends to be slower than that of younger men. This difference in overall speed is not due to slower movements so much as to longer times spent at points where the movement direction must be altered.

It seems reasonable to make a more general statement in the form of the following hypothesis. If the activity is such as to impose no undue stress, the difference between the performance of older and younger groups of people is due to extra time which the former use to absorb information at a central level rather than to time taken up directly because of some deterioration of the peripheral receptor and effector processes.

The phenomena discussed in this paper may be related to several others which have been revealed by the improved recording techniques available in recent years. Rubin, von Tebra & Smith (1952) have reported that '. . . it is mainly the manipulation component of a motion pattern that is influenced by practice and not the travel component'. Seymour (1954) has suggested that '. . . increased speed of performance in accomplishing such industrial tasks (simple manual skills) results not so much from faster movement as from

a briefer duration of stationary therbligs or elements'. In relation to certain manipulative skills, Singleton (1953) wrote that 'the important determinant of performance and indicator of fatigue is not the time required to execute movements, but the time between movements'. All the experiments reported or referred to in this paper indicate the importance of separating into their various components the total times of perceptual-motor tasks. In particular, they point to the crucial role which the time occupied by central mechanism plays in the performance of such skills.

I am indebted to Mr A. T. Welford for advice on the work described in this paper. I should also like to thank Mr L. V. Green of the Dunlop Rubber Company and the staff of the Dunlop Cotton Mills, who arranged for and acted as subjects in the Two-choice experiment.

References

Leonard, J. A. (1952). Some experiments on the temporal relation between information and action. Unpublished Ph.D. Thesis, Cambridge University.

Miles, W. R. (1931). Measures of certain human abilities throughout the life span. *Proc. Nat. Acad. Sci., Wash.*, xvii, 627–33.

Rubin, R., von Tebra, P. & Smith, K. V. (1952). Dimensional analysis of motion. III. Complexity of movement of pattern. *J. Appl. Psychol.* xxxvi, 272–6.

Seymour, W. D. (1954). Manual skills and industrial productivity. *J. Instn Prod. Engrs.*

Singleton, W. T. (1953). Deterioration of performance on a perceptual motor task. Pp. 163–72 in W. F. Floyd and A. T. Welford (ed.) *Symposium on Fatigue.* London: H. K. Lewis and Co. Ltd.

Szafran, J. (1951). Changes with age and with exclusion of vision in performance at an aiming task. *Quart. J. Exp. Psychol.* iii, 111–18.

Welford, N. T. (1952). An electronic digital recording machine the S.E.T.A.R. *J. Sci. Instrum.* xxix, 1–4.

(*Manuscript received* 9 *February* 1954)

COGNITIVE CHANGES IN THE FEEBLEMINDED[1]

By A. D. B. CLARKE and A. M. CLARKE

Psychology Department, The Manor Hospital, Epsom

I. *Introduction* (p. 173). II. *Frequency of* I.Q. *changes* (pp. 173–175). III. *Hypotheses to explain results* (p. 175). IV. *The hypothesis of adverse environmental influence* (pp. 175–177). V. *Conclusions* (pp. 177–179). *References* (p. 179).

Introduction

An earlier paper by the writers (1953) has discussed the general question of I.Q. variability, and evidence was presented indicating that during the period of mental growth I.Q. constancy for the individual over long periods of time is the exception rather than the rule. The thesis was advanced that the I.Q. must be regarded as an objective and relatively reliable estimate of a person's present intellectual status, but that for predictive purposes, even though better than subjective judgement, the results of intelligence tests will be unreliable for a considerable proportion of individuals. Thus, Honzik, MacFarlane & Allen (1948) found that only 15% of a group of 153 children changed in I.Q. by 9 points or less between the age of 6 and 18, while 58% changed 15 or more I.Q. points.

The present paper examines in detail an investigation of I.Q. changes in the certified feebleminded. It began with the observation that among a group of eighteen adolescent and young adult patients there had occurred in some cases quite large increases in I.Q. on the Wechsler Test, after a time interval of about 18 months. These changes ranged from a decrease of 5 to an increase of 25 points upon retest, with a mean increase of 9·3, S.D. 7·3. A control group of eighteen patients, all new admissions, was matched with the first group for initial I.Q. and age, and retested after a time-interval of only 3 months, in order to determine the maximal effect of test practice, errors of measurement and underestimation at the first test. It was found that an average increment of only 4 points occurred under these conditions, this being significantly lower than the increase shown by the first group.

It was further noted that although researches existed in which I.Q. changes in mental defectives had been studied, rarely had such workers as Burt (1947), McKay (1942), Roberts (1945), Spaulding (1946) and Guertin (1949) done anything more than to demonstrate that such changes occur, and in some cases to suggest hypotheses to explain irregularity of intellectual growth. In most cases these hypotheses seem never to have been submitted to experimental verification.

II. Frequency of I.Q. changes

Since it was known that the first small group was not a representative sample of the feebleminded hospital population, it was decided to plan a research using a larger number of patients to determine how frequently I.Q. changes could be expected to occur among the younger certified feebleminded, and, more important, why such changes take place.

As a preliminary, a note was made of every patient who had been first tested with the

[1] This article is based upon papers read to the Mental Deficiency Section, The Royal Medico-Psychological Association, 5 November 1952, and to the British Psychological Society, 13 April 1953.

Wechsler about 2 years previously, and a detailed examination of reasons for referral showed that the fifty-nine patients still available were a representative sample of the younger feebleminded hospital population. These individuals were then retested, and Table 1 shows the results. It will be noted that the increase was significant beyond the 0·1% level of confidence. It should be further noted that the increments were general throughout all the subtests of the Wechsler Scale, apart from Digit Span, where the increase did not reach the 5% level of significance. It had been predicted that there would be a greater increase on the performance items than on the verbal subtests. The mean verbal weighted score increase was 3·203, S.D. 4·405, while that for the performance subtests was 4·729, S.D. 6·161. The difference between these means is, however, only just significant at the 5% level, using the one-tail test.

Table 1. *Results of test and retest on Wechsler, Form I, for fifty-nine patients retested after an average interval of* 27 *months*

	Original full scale I.Q.	Retest full scale I.Q.	Difference
Mean	66·2	72·7	6·5
S.D.	14·0	13·4	6·2
N	59	59	59

t (correlated means) = 8·074, significant beyond the 0·1% level

I.Q. range at original test:	35 to 98
I.Q. range at retest:	40 to 97
Age range at retest:	14 to 50 years, mean 23·5, S.D. 8·1*
Period between test and retest:	mean 27 months, S.D. 6 months.
Range of I.Q. changes:	−7 to +25 I.Q. points.

* Only nine patients were over 30 years of age.

It was felt necessary to augment the control group, and therefore a further eleven patients were retested after a short time-interval; for this larger control group of twenty-nine patients the mean increase was 4·1, S.D. 3·8 I.Q. points, this increase being significant at the 0·1% level. A comparison of the distribution of changes in the main group and the control group will be found in Table 2.

Table 2. *Comparison of* I.Q. *changes for the main group and control group*

Change	Main group		Control group	
− 7 to 0 I.Q. points	9/59 = 15%	100%	4/29 = 14%	100%
+ 1 to +7 I.Q. points	23/59 = 39%		19/29 = 65%	
+ 8 I.Q. points or more	27/59 = 46%		6/29 = 21%	
+10 I.Q. points or more	17/59 = 29%		1/29 = 3%	
+15 I.Q. points or more	7/59 = 12%		0	
+25 I.Q. points	1/59 = 2%		0	

Inspection of the Table shows clearly the greater gains made by the members of the main group. The first question raised at the outset can now be answered; almost half of the patients restested after 2 years showed gains of 8 points or more, this figure having been chosen as the base-line for significant cognitive improvement. It is obvious, moreover, that for the majority of such patients the increments occurring over this time-interval do not represent the total change which the individual has already made or will make in the future. Honzik *et al.* (1948), for example, have pointed out that some subjects show consistent trends in increment or decrement over many years. The results

thus give a picture of a considerable proportion of the certified feebleminded advancing towards intellectual normality at ages when mental growth is commonly assumed to have ceased.

III. Hypotheses to explain results

The second important research problem was to determine the reason for these increments, and the following main hypotheses were investigated:

(1) That the results can be explained entirely in terms of test practice, errors of measurement and underestimation at the time of the first test. The results from the control group of twenty-nine patients retested after an average of 3 months, who showed only a 4-point increment on the average, dispose of this hypothesis.

(2) That those individuals who made the big increases would be those who were new admissions to the hospital at the time of the first test and were underestimated since admission may represent a traumatic experience for the individual. It was found that the increments of those first tested as new admissions were no greater than for those who had been in the institution for a considerable time before the first test. Furthermore, the control group of patients retested after an average of 3 months were mostly new admissions examined within a few days of arrival.

(3) That those who increased would be the younger members of the sample. It was found that the correlation between age and weighted score increment was $-0{\cdot}155$, which is in the expected direction but is not significant.

(4) That there would be a relationship between initial I.Q. and subsequent change. The correlation between full-scale I.Q. on the first test and subsequent change is $-0{\cdot}311$, significant at the 2% level of confidence. There is, therefore, a small but significant tendency for those with lower initial I.Q.'s to make larger increases than those with higher initial I.Q.'s.

(5) The final hypothesis, which had emerged from inspection of the data concerning the first group of eighteen cases, was that those whose condition, irrespective of possible genetic causes, seemed to be related to early adverse environment, would be those in whom the larger I.Q. increases occurred.

IV. The hypothesis of adverse environmental influence

The first method used to investigate this final hypothesis involved an independent analysis of all documents relating to each patient, and an attempt was made to determine the aetiology of the deficiency in each case. The categories included disease, adverse environment, heredity and various combinations of these. The I.Q. increments were then plotted for the members of each aetiological group and all were insignificantly different from one another, apart from the disease group for which the mean increment, as might be expected, was only half an I.Q. point. There were, however, only four patients assigned to this group, a number, of course, too small to warrant general conclusions. The failure of aetiological classifications to correlate with cognitive changes could be the effect of either of the following factors: first, that, as Penrose (1949) points out, it is extremely difficult to determine the precise aetiology of mental deficiency, except in the relatively few clear-cut cases of disease and low-grade mental defect. The problem of aetiological classification becomes apparent when one remembers that while the incidence of severe defect is equal throughout society, the feebleminded, who form the vast majority of

mental defectives, are to be found mainly in the socially deprived groups in which it might be argued that both adverse heredity and adverse environment may be the factors responsible. Secondly, that, in fact, cognitive improvement can be expected in all feebleminded patients irrespective of aetiology, with the probable exception of those whose condition is attributed to disease or injury.

On the basis of previous work the latter of these two possibilities seemed to be unlikely and therefore further work was undertaken to elucidate the failure of aetiological classification to be of significance. It was argued that there must be factors related to these I.Q. changes, either in the past or the present histories of the patients concerned. There appeared to be nothing relevant in the present history, so once again all records of past history were examined, and all specific factors which could possibly be relevant were separately tabulated: e.g. number of sibs, illegitimacy, parental separation, parents said to be of poor mentality, early separation from the parents, institutional upbringing, N.S.P.C.C. reports, absence from school, illness, etc. Inspection of the results seemed to show that certain indications of an early bad home, irrespective of possible genetic factors, corresponded with the amount of I.Q. change. An attempt was therefore made to formulate valid criteria of early adverse environment. These are given in rough order of importance:

(1) N.S.P.C.C. or Crusade of Rescue adverse reports or intervention, or statement in the case notes 'Family known to the N.S.P.C.C.'.
(2) Parental attitude to the child said to be antagonistic or uninterested. Parental cruelty and fear of the parents by the child.
(3) No fixed abode. Living in shelters with the parents or on the street.
(4) Orders appointing someone as 'Fit Person' to take charge of the child. This normally means the removal of the child from the parent by some authority.
(5) Statement in the report that 'Home conditions are bad' or 'very bad'.
(6) Statement in the report that the child has been considerably neglected.
(7) Irregular attendance at school due to parental neglect or lack of interest.
(8) Home said to be dirty and neglected.
(9) Gross poverty.
(10) History of crime in parents or sibs, gross intemperance, etc.
(11) Dietary deficiency: rickets.
(12) Child found begging or wandering or missing from home.

The presence of two or more of these criteria was taken as evidence of a very bad home, and in most cases thus classified several of these somewhat overlapping criteria were present. This may be illustrated by the bare outlines of two typical case histories:

(*a*) Amy and her twin sister, now aged 18, were admitted to a hospital with measles and a wasting disease at the age of 18 months. At the age of 3 Amy was placed in a convent as a resident owing to the poverty of the family. She was removed at the age of 12 against the advice of the Crusade of Rescue. The mother ill-treated both twins badly, and the school reports state that they suffered insidious torment at home; they were sent to school in a shocking condition, and when the teachers supplied clothes to replace the children's rags, these clothes had to be kept at school or the mother would remove them. Amy's progress at school was very adversely affected by these circumstances, and the child herself was pitiful in her requests to be taken away from home.

(*b*) Stanley, now aged 21, was said to have had 'fits' between 1935 and 1940, but there are no details of them. He suffered from rickets as a child; school reports state that his educational attainments were very poor, but he was obedient and quiet. He wandered, and school attendance was very bad. At the age of 12 he was brought before the Juvenile Court as being in need of care and protection. He was said

to be terrified of his father, and was never allowed to have enough sleep. The parents were that year prosecuted for his non-attendance at school. The father was stated to be of low mentality, the home to be dirty and neglected, and the mother to be illiterate, callous and intemperate. The parents separated shortly thereafter.

When these twelve criteria had been formulated, they were applied to all fifty-nine case histories, and it was found that those with very bad homes were mainly those who had made the larger I.Q. increments. In order to check upon this finding, and to exclude any possibility of subjective bias, it was necessary to call in an outside investigator to carry out the ratings independently, and this Dr J. Tizard of the Unit for Research in Occupational Adaptation, Medical Research Council, very kindly consented to do. Having no knowledge whatever of either the patients or the I.Q. changes which they had made, Dr Tizard analysed the contents of all the fifty-nine case histories with the twelve criteria in mind, and assessed presence or absence of very bad homes. During the 3 days when this task was in progress, there was no discussion whatever of the cases or of the research between Dr Tizard and the writers.

Upon completion it was found that there was agreement on fifty-five out of the fifty-nine cases; in the four instances where there was disagreement, Dr Tizard's judgement was left unaltered and accepted as final. There were thus two groups of patients, those whom the external investigator considered to have very bad homes and those against whom there was either no evidence, or the evidence was inconclusive, or there existed evidence pointing to good homes. Details of the I.Q. increments for the members of the two groups are shown in Table 3, together with control group data.

Table 3. *Changes in* I.Q. *points*

	Very bad homes	The remainder	Control group
Mean change	+9·7	+4·1	+4·1
S.D.	6·3	4·9	3·8
N	25	34	29

t (uncorrelated means) = 3·864, significant beyond the 0·1% level.

Distribution of changes

Change	Very bad homes		The remainder	
− 7 to 0 I.Q. points	1/25 = 4%		8/34 = 24%	
+ 1 to +7 I.Q. points	6/25 = 24%	100%	17/34 = 50%	100%
+ 8 I.Q. points or more	18/25 = 72%		9/34 = 26%	
+10 I.Q. points or more	12/25 = 48%		5/34 = 15%	
+15 I.Q. points or more	7/25 = 28%		0	
+25 I.Q. points	1/25 = 4%		0	

The difference between the gains made by the two groups is significant at the 0·1% level. It will be noted further that the group of thirty-four patients who were not classified as having had very bad homes made the same sort of gains as the members of the control group; in other words, their increments were of the order one would expect from the maximal effects of practice, errors of measurement and possible underestimation at the time of the first test.

V. Conclusions

These results show clearly two things: first, the very close agreement, 93%, between independent ratings of bad home points to the objectivity of such assessments; and secondly, that these criteria are effective in discriminating on the basis of early history between those who later in life had I.Q. increments, and those who showed no such large changes.

It should be remembered that the majority of those who are certified as feebleminded come from what the man-in-the-street would consider to be bad homes. In the present sample of fifty-nine individuals, for instance, in only six cases was there evidence of good homes. What the present investigation has shown, therefore, is that in a population in general drawn from bad homes those with the worst are distinguished from those with the less bad homes by their later cognitive improvement. It is consistent with this main finding to suggest that the environment which is really antagonistic towards the child retards mental development for many years. Later, however, this retardation begins to fade, I.Q. increments thus occurring. There is as yet no certain evidence to show whether this is the effect of delayed maturation or of the relatively better and more stable environment of the institution, or a combination of both. In view of the fact that length of time in the institution is not associated with the increases, it seems likely that these changes are more the effect of removal from a very adverse environment than of entry into a relatively better one.

Retrospective validation such as has been described carries with it the possibility of error by capitalizing upon chance relations existing within the records of those investigated. It was necessary, therefore, to find out whether, by using the twelve criteria of very bad homes, it was as possible to predict I.Q. increments as it had been to discriminate retrospectively. From the twenty patients who had been first tested during the first few months of 1951 and who had not yet been retested as members of the control group, eleven were selected, consisting of those in whose records there were clear indications of very bad homes and those in whom this was contra-indicated. Before any retesting was carried out, definite predictions were made that there either would be or would not be a significant increment. Six patients for whom increments had been predicted showed increases on retest of: +14, +9, +10, +10, +4 and +5, with a mean of 8·7 I.Q. points, and the five in whom increments had been contra-indicated showed the following changes: −5, −2, +7, −1, +4, with a mean of 0·6 of an I.Q. point. On this very small sample, therefore, prediction based on the criteria is fairly accurate. It is, of course, intended later to check this finding independently on a larger sample.

The findings can now be summarized and the implications considered. The results must be seen against the background of the several hundred researches which indicate I.Q. variability as a normal feature over considerable lengths of time. In this investigation, however, the changes which occurred were mostly in the upward direction and were often large in relation to the relatively short time-interval of about 2 years between test and retest, almost half of the fifty-nine subjects having increased by 8 points or more, the maximum individual increment being 25 points.

While many previous researches have suggested hypotheses to account for I.Q. variability, seldom have such hypotheses been carried to the stage of experimental verification. An attempt was made in the present investigation to remedy this defect. It was shown that I.Q. increments are associated with early very adverse environment, and that intellectual subnormality among such deprived people as the certified feebleminded is not necessarily a permanent and irreversible condition. Obviously in many cases the increments in I.Q. over a period of 2 years do not represent the total change for the individual.

The main question which this research has raised is as follows: having shown that I.Q. increments occur in a large proportion of the certified feebleminded, and that these

increases are associated with early deprivation, an investigation must now be planned to find out how such relatively spontaneous changes can be both accelerated and improved.

The writers wish to thank Dr J. F. MacMahon, Physician Superintendent, for his very great help and encouragement during the entire course of the research. Dr J. Tizard, M.R.C. Unit for Research in Occupational Adaptation, played a vital part in one aspect of the investigation, and Mr M. B. Shapiro, Senior Lecturer, Psychology Department, Institute of Psychiatry (Maudsley Hospital) made stimulating suggestions and criticisms. Mr A. Ehrenburg, Statistical Laboratory, Maudsley Hospital, gave useful advice, and Mr G. J. F. Singleton kindly performed the statistical computations.

References

Burt, C. (1947). *Mental and Scholastic Tests.* London: Staples Press.
Clarke, A. D. B. & Clarke, A. M. (1953). *Lancet,* II, 877.
Guertin, W. H. (1949). *J. Clin. Psychol.* v, 414.
Honzik, M. P., MacFarlane, J. W. & Allen, L. (1948). *J. Exp. Educ.* xvii, 309.
McKay, B. E. (1942). *Amer. J. Ment. Defic.* xlvi, 496.
Penrose, L. S. (1949). *The Biology of Mental Defect.* London: Sidgwick and Jackson, Ltd.
Roberts, A. D. (1945). *Amer. J. Ment. Defic.* l, 134.
Spaulding, P. J. (1946). *Amer. J. Ment. Defic.* li, 35.

(*Manuscript received* 8 *September* 1953)

A THEORY OF HUMAN CURIOSITY

By D. E. BERLYNE
University of Aberdeen

I. *Introduction* (pp. 180–181). II. *The sequence of events* (pp. 182–184). III. *The role of conflict* (pp. 184–189). IV. *Summary* (pp. 189–190). *References* (pp. 190–191).

I. Introduction

Few phenomena have been the subject of more protracted discussion than human knowledge. Yet this discussion has usually paid little attention to the motivation underlying the quest for knowledge, with the result that two important questions still confront us. The first question is why human beings devote so much time and effort to the acquisition of knowledge. Sometimes there is some obvious drive to whose satisfaction it can contribute. But, strangely enough, many of the queries that inspire the most persistent searches for answers and the greatest distress when answers are not forthcoming are of no manifest practical value or urgency. One has only to consider some of the ontological inquiries of metaphysicians or the frenzy of crossword enthusiasts to be convinced of this. The second question, and the main concern of the present article, is why, out of the infinite range of knowable items in the universe, certain pieces of knowledge are more ardently sought and more readily retained than others.

Modern learning theory leads us to look for motivational variables to answer these questions, and a drive which is reduced by the reception and subsequent rehearsal of knowledge is what we generally call 'curiosity'. However, we must draw a distinction between this curiosity and the curiosity drive that has been studied in lower animals (Berlyne, 1950). In the case of the rat, for example, there appears to be a drive which is aroused by novel stimuli and reduced by continued exposure to these stimuli. Its reduction reinforces exploratory activity, i.e. activity, such as approaching and examining the stimulus-objects, which increases stimulation of the animal's receptors by them. Now, similar exploration is undoubtedly elicited by strange objects in adult and especially infant human beings. But in an animal as well endowed for learning and remembering as the human adult, exploration is bound to leave a stock of permanent traces in the form of symbolic representations ('pure stimulus acts' or 'cue-producing responses'), which are manifestations of what we call 'knowledge'.

The curiosity which leads to increased perception of stimuli and the curiosity whose main fruits are knowledge may well turn out to be closely related. But, as we are using different defining operations for them, we shall have provisionally to use two different terms. We shall therefore call the first '*perceptual curiosity*' and the second, which is our concern in this article, '*epistemic curiosity*'.

An account in behaviour-theory terms of the nature and origins of knowledge has been offered elsewhere (Berlyne, 1954). According to this account, which draws on concepts introduced by Morris (1946) and by Osgood (1952), knowledge consists of *habits mediating believed, designative symbols*, which form sequences (trains of thought). The actual course followed by a train of thought is determined jointly by (1) *cue-stimuli*, which include

external stimuli (S) and the self-stimulation (s) resulting from previous items in the sequence and (2) *motivational stimuli*, which include drive-stimuli (S_D) and the goal-stimuli (s_G) produced by fractional anticipatory goal-responses (r_G).

The learning that produces knowledge can clearly be biologically helpful because (1) it can enable goal-directed behaviour to be more efficient through being better prepared for what is impending, and (2) it can enable warning signals to be recognized, so that danger can be avoided (Mowrer, 1939). But these effects are usually delayed, so that their advantages do not explain what reinforces the learning in question. For long-term consequences to influence behaviour, it is necessary for symbols to be used in such a way that the reinforcements of fear-reduction and secondary rewards can be brought to bear (Mowrer & Ullman, 1945). The drives that are reduced by knowledge are thus largely the 'coexistent emotional components', which, as Ullman (1951) argues, we must assume to be present in all primary drives and to be capable of functioning anticipatorily. These components are what are called 'fears'—Ullman speaks of 'shock-fear' and 'hunger-fear', for example—and knowledge can lead to fear-reduction in various ways: (1) by depicting the future situation as a desirable one (reassurance), (2) by reducing 'fear of fear' or 'fear from a sense of helplessness' (preparation) (Mowrer & Viek, 1948), or even when the outlook is hopeless, (3) by reducing the increment of drive (conflict) due to uncertainty (the comfort of 'knowing the worst'), or (4) extinction of fear by repeated exposure to frightening stimuli ('getting used' to unpleasant prospects).

But our main concern is with the second question, viz. the question of the factors underlying the selectivity of epistemic curiosity. Why does an individual seek or learn one piece of knowledge rather than another? Representatives of various schools of psychology have provided hints but scarcely more:

(1) *Psychoanalysis.* The writings of Freud (e.g. 1905) and his followers (e.g. Abraham, 1927) make it clear that psychoanalysts would attribute the desire to know to any of several 'component drives' of the libido—scoptophilic, oral-incorporative, oral-sadistic, anal-aggressive or anal-retentive—according to the direction it takes. But this leaves many questions unanswered. How are we to predict when one of those 'component drives' will find an outlet in curiosity, and how intensely? And which particular items of knowledge will be sought?

(2) *Gestalt psychology.* Although the Gestalt psychologists have not produced a systematic account of curiosity, it is not difficult to guess how such an account would go. They explain much of behaviour by the '*principle of closure*', the tendency to act in such a way as to close a 'gap', whether in a perceived figure or in some other aspect of the 'behavioural world' (Koffka, 1935; Wertheimer, 1945). It is evident that curiosity consists precisely of a drive to fill in such gaps in the subject's experienced representations. But again, we have no definition precise enough to tell us infallibly what will constitute a 'gap', nor which gaps will have precedence over others.

(3) *Reinforcement theory.* The tendency to acquire the verbal or other responses which constitute knowledge is a product of learning, culturally conditioned, according to such reinforcement-theorists as have considered the problem. Dollard & Miller (1950), mention learned drives to 'make a correct report of the environment' and to 'have an explanation' and the punishment that social training, as well as the demands of reality, imposes on those who fail to do so. Skinner (1947) similarly describes how a child learns to emit 'tacts' (i.e. verbal responses controlled by properties of objects or situations)

under the influence of 'generalized reinforcers', particularly approval. Mowrer (1950) appears to identify the acquisition of 'beliefs' (p. 5) and 'knowing that' (p. 268) with the conditioning of emotional responses, but this does not acknowledge the role of symbolic responses in distinguishing pieces of knowledge with similar affective value but different content.

We shall take these treatments as a starting-point, although it is clear that they leave some essential questions unsettled. There is, for one thing, the paradoxical fact that curiosity seems to be evoked most uniformly by situations that are new and strange. They would be the last we should expect to have any influence at all, if it were a matter of generalization from prior training (Berlyne, 1950, p. 71). It will be our contention that conflict supplies the clue to these cases.

II. The sequence of events

Stimuli which are used to elicit verbal behaviour, unless they resemble the behaviour they call forth or have unique responses, are what Skinner calls '*thematic probes*' (1953, 1947). They can take the form of verbal or non-verbal stimuli, and they can be administered to oneself ('self-probes'), as a reaction to a perceived situation, or come from outside in the form of writing, speech or non-linguistic cues. We can extend Skinner's concept a little, but not, it is hoped, inexcusably, by including under it all stimuli which elicit trains of thought, whether verbal in content or not.

Skinner gives as illustrative cases of thematic probes the stimulus-words of association experiments and the material used in projection tests. But it has been known since the work of the Würzburg school (Ach, 1910) and of Lewin (1917, 1922) that it is not possible to predict what association, if any, will be given to a stimulus without taking into account the 'set', 'determining tendency', 'tension-system', etc., induced by other stimuli, usually instructions. In other words, we must have not only cue-stimuli to act as a starting-point, but also motivational stimuli to limit the responses to the general category required by the task on hand and to supply the persistent motive force for the process. The thematic probe must thus have two parts or aspects with these distinct functions, and the clearest example, as well as probably the commonest in practice, is the *question*. The question of the type called by linguists the 'specific interrogation' (as distinct from the 'yes-or-no question') (Bloch & Trager, 1942) has the two parts easily distinguishable. As an example, we may take the question, 'how does the starfish eat?' We assume that the question, in common with all synonymous questions, evokes mediating 'concepts' or 'meaning' responses (r_m) (Berlyne, 1954). The meanings corresponding to 'starfish eat' act as the cue-stimuli with a patterning effect peculiar to that stimulus-complex; in some cases, such as when the question is put by an authoritative person, they may be tantamount to an assertion that the starfish eats, while in other circumstances the question may be taken to mean 'how, if at all, does the starfish eat?' The group of concepts that act as cue-stimuli we may, following Morris's terminology, call the '*designator*'. On the other hand, the interrogative adverb 'how' produces a meaning which acts as a motivational stimulus. It limits the train of thought to 'how-concepts' and evokes a learned drive-state which motivates the reaction.

When a question is put, whether by the subject himself or by somebody else, and the answer is already known, the appropriate response is made as a reaction conditioned by previous learning to the stimulus-pattern, and this relieves the drive immediately, so

that the subject can proceed to some other activity. However, when the answer is not known, the drive will persist, and some sort of trial-and-error process can be expected to follow as with any other drive-state. Of course, the trial-and-error will not be completely random, as it is not even for the rat in the Skinner-box: it will take the form of behaviour resembling what has succeeded in similar situations. The most likely behaviour-sequences to occur are: (*a*) *thinking*—implicit trial-and-error, insightful restructuring (Hull, 1952, chap. 10), stimulus generalization (as in 'deduction' Mowrer, 1950, chap. 11), 'intuition' (Piaget, 1947) and 'magical thinking' (Fenichel, 1945, p. 47); (*b*) *observation*—approach, receptor-adjustment, manipulation of environment, so as to perceive relevant stimuli, culminating in the controlled experimental and other techniques of science; (*c*) *recourse to authority*—asking experts, consulting books or oracles, etc. (Cf. intuitionism, rationalism, empiricism, and authoritarianism (Montague, 1936).)

If the processes lead to a pattern of responses that the subject's prior learning enables him to accept as an adequate answer, then the drive will be reduced. Since drive-reduction follows the rehearsal of the correct answer, the principle of reinforcement (Hull, 1952, postulate IV) implies that the latter will become strengthened as a response to the question. Furthermore, by the reinforcement-gradient principle, it will be learned more strongly than the responses that led up to it, so that in future the question will be followed by the answer immediately, and intermediate steps will be omitted.

If the answer is not arrived at readily by any of the procedures mentioned, then the process may be brought to an end in other ways. Some distraction may occur, i.e. an incompatible response-tendency with a higher reaction-potential may arise, or extinction may supervene. There will be extinction of each line of inquiry as it turns out to be unsuccessful and gives way to another, exactly as in trial-and-error learning (Hull, 1930), and also ultimately the drive-producing responses may be extinguished, so that the subject gives up altogether. It is unlikely that extinction will affect the intervening link, that between the words and the drive, since the motivating power of interrogative adverbs is frequently and partially reinforced in everyday life.

Let us now suppose that the subject fails to hit upon the correct answer in the course of striving for it. And let us suppose that on some future occasion he is told or shown the answer, i.e. exposed to some stimulus-complex which evokes the response he was seeking. We can expect this answer to elicit, by ordinary redintegrative remembering, an internal rehearsal of the question, so that it is *recognized* as the answer he was looking for on the earlier occasions. The stimuli produced by the response of rehearsing the question will thus occur about the same time as the rehearsal of the answer, and the stimuli produced by rehearsal of the answer will be followed closely by reduction of the drive that the question has re-aroused. Thus we can see the answer being learned by reinforcement as a response to the question, so that a new piece of knowledge is acquired.

It will by now be evident that the drive aroused by questions and other thematic probes is, by our definition, a form of epistemic curiosity. And an important consequence follows from the principles of behaviour theory, if our account so far is valid, which gives us a way of measuring this curiosity through its effects on remembering. Both introspective and behavioural evidence reveal that, when an acceptable answer to a question has been encountered and rehearsed, curiosity is reduced to a subthreshold value. But the higher the drive before such reduction, the greater the *amount of reinforcement* or quantity of drive-reduction (K). And according to Hull's (1943, 1952) postulates, the

probability of a response occurring on future occasions increases with reaction-potential (S^ER), which in its turn increases with K. It follows that those questions which originally aroused most curiosity are most likely to be answered correctly if they are encountered again after the answer has been presented to the subject, and we can use the probability of recall as a measure of curiosity. An additional measure depends on the fact that subjects are likely to have learned to respond with tacts to their own internal stimuli (Skinner, 1953, 1947), although less accurately than to external stimuli. They can accordingly be instructed to indicate which questions arouse the greatest desire to know the answer.

We have therefore arrived at the hypothesis that curiosity is aroused in a subject when a question is put to him, whether by himself or by an external agent. Some component (s_{mD}) of the response-produced stimulation resulting from the meaning of the question (r_m) is assumed to act as a drive-stimulus. And we can see that the intensity of this drive-stimulus, which will in its turn depend on the amplitude of the response (r_{mH}) that produces it, will be one of the most important variables affecting the drive strength of the curiosity.

There is some experimental evidence for the curiosity-inducing role of questions, but it is also borne out by everyday experience. Many celebrated thinkers have been stimulated to a lifetime's meditation simply because they thought of new questions about matters that ordinary men have taken for granted. Similarly, the skilful lecturer excites curiosity in his audience by putting questions to them, perhaps about familiar phenomena, which it has never occurred to them to ask themselves.

However, the factors mentioned so far do not adequately explain the most striking cases of curiosity-arousal, those concerning the strange, the unusual, the puzzling. To attempt an explanation of this side of human nature, we shall have recourse to another variable, *conflict*.

III. The role of conflict

After the necessary preliminary phase of considering over-simplified situations, in which either only one response-tendency or motive is active or else one response-tendency or motive is so much stronger than others as to be virtually alone in its influence, psychological theory had to turn to more realistic situations where there are factors in competition. Even an elementary treatment of trial-and-error learning (Hull, 1930) forces us to consider the process whereby one response overcomes alternative ways of reacting, but special phenomena result when competing tendencies are fairly evenly matched in strength. The study of such phenomena was begun by Lewin (1935) and then carried further on both theoretical and experimental planes by Miller (1944, 1951) and his associates. Dollard & Miller (1950) have shown how the behaviour-theory of conflict can be extended to embrace the main effects ascribed to conflict by Freud, while Hull has endeavoured to reveal its roots in the basic principles of learning (1952, chap. 8). A theory of emotion, based on the assumption that conflict (F) is in itself drive-producing, is an important recent development for which Brown & Farber (1951) are responsible and there are various observations from experiments with rats that tend to confirm this assumption (Finger, 1941; Lowell, 1952; Miller & Stevenson, 1936).

A rather different recent emphasis on conflict has come from Hebb (1946, 1949). This is particularly deserving of mention here, because it involves the central processes inter-

vening between stimulus and response, and that is precisely where we must seek the kernel of curiosity. Behaviour, in Hebb's view, depends on the intricate and nicely timed co-operation between 'cell-assemblies' in the cerebral cortex. If the timing goes awry, or if the processes ('phase-sequences') in the cortex otherwise interfere with one another, disruption will result. Some phase-sequences require the support of externally initiated sensory processes, and if these are not forthcoming, as when something familiar with an unexpected feature is perceived, disruption is once again a likely outcome. This disruption, which leads to a diffuse and disorganized release of energy, is what, according to Hebb, lies behind emotion. His principal illustration is his description of the fear induced in chimpanzees by surprising sights, but it is easy to see that these sights might instead have aroused curiosity in slightly different conditions. In his treatment of perceptual learning, Hebb describes how repeated exposure to a complex of stimuli builds up integrated and organized patterns of activity in the cortex, and thus conflict is eliminated as the unfamiliar becomes familiar. If we admit the possibility that the curiosity aroused by unusual perceptions has something to do with conflict, then the elimination of this conflict by exploration and the consequent drive-reduction might well play a part in perceptual curiosity. If we then extend these ideas to the autonomous processes which are the result of prior learning but can later run off in the absence of the corresponding environmental events, we can readily imagine how strange and puzzling thoughts or concepts may likewise involve conflict, and the acquisition of knowledge may mean the formation of new structures which obviate this interference. If conflict is a drive, the reduction of conflict will be reinforcing, and it will provide the explanation for the reward-value of investigating things that are puzzling and the learning of knowledge resulting from this investigation. Epistemic curiosity also will thus be attributable in many cases to a similar mechanism.

Hebb's concepts are physiological and refer to neural processes. But since these processes are at present not observable and serve merely as devices for explaining what can be observed, they are best regarded as intervening variables. It should not be difficult therefore to translate them into behavioural terms. The preference for purely behavioural terms may be justified as more than a matter of verbal taste, since these terms point to relationships between the sort of conflict under discussion and other areas in behaviour theory, including other forms of conflict.

We can begin our inquiry into the conflicts affecting trains of thought by recalling Miller's (1944) list of ways in which responses may be incompatible and therefore conflict. Sometimes the incompatibility is physical and innate, like that between approaching and avoiding the same object. But at other times, the conflict is learned. The responses are not inherently antagonistic, but learning has made the organism unlikely to perform both simultaneously or in close succession. This means that the response-produced stimulus (s_1) resulting from the first response (R_1) has become conditioned to a response (R_{-2}) which is physically incompatible with the second (R_2). If R_{-2} is stronger than R_2, the latter will thus be inhibited. When we extend these notions to symbolic responses, it is clear that physical incompatibility will not be of major importance. It may be that certain perceptual responses are innately incompatible, so that the conflict between an expectancy and a perception, for example, may be unlearned. But most of the antagonisms between symbols are almost certainly the results of learning, which trains us not to apply two particular words to the same object or combine two particular

concepts (r_m) in the same complex. Thus a thought or a perception may conflict with past experience by incorporating two elements previously learned as incompatible. There are two cases of learned incompatibility that are particularly likely to affect trains of thought. One is the learned incompatibility between *contradictory* beliefs, which enables us to recognize and avoid fallacious symbol-sequences. The other is the conflict between the learned fear of *irrelevance* and the tendency to perform an irrelevant verbal response. As we have already noted, the Würzburg school were impressed with the way in which the determining tendency kept thought in the right direction by excluding irrelevant associations. Modern learning theory (Dollard & Miller, 1950) leads us to the conclusion that this happens because the emergence of an irrelevant thought evokes a learned drive and its inhibition reduces the drive and is consequently reinforced. The existence of a strong 'drive to be relevant', at least in our culture, can be demonstrated in almost any session of psychotherapy with a neurotic. Long before his free association has led him to touch on anything delicate, he shows resistance, due to his previous training to speak relevantly and coherently. It takes several sessions before he can flit inconsequentially from one topic to another or expatiate on matters that seem unconnected with his symptoms, as he must if he is to obey the 'basic rule'.

In order to show how conflict can affect curiosity, it is helpful once again to take a concrete example of a question: 'what crops do some ants cultivate in underground "farms"?' We have already introduced the assumption that a question arouses concepts (r_m), including some which are drive-producing. But, in addition, we can indicate, with the aid of our illustrative question, four stages, at any or all of which conflict may occur to bring about an increment of curiosity-drive:

(1) The question itself may evoke concepts which past experience and instruction have made incompatible for the subject. In the case of our example, learned conflict may well exist in a zoologically naïve person between all concepts relating to farming and all relating to subhuman animals. This is what happens when the designator of the question is said to be 'surprising' or 'unexpected' or 'strange' or 'puzzling'. Instead of such a question being put to the subject by an outside source, he might have come across a stimulus-situation arousing perceptual responses which he has been trained to regard as incompatible. This might lead him to take a closer look (perceptual curiosity) or to formulate such a question himself.

(2) Even if the question itself did not imply any surprising fact (e.g., 'how does the starfish eat?') conflict may well arise immediately after its formulation, if the answer is unknown. If the answer is known, then there will be a response already learned to the stimulus-pattern produced by the question, and this response will probably occur without delay. But if the answer is unknown, then we must turn to behaviour theory for some hints as to what might ensue. If the organism is confronted with a combination of stimuli to which it has learned no response, we shall not expect it to do just nothing, unless it is a very primitive or young organism indeed. As Hebb (1949) has aptly reminded us, even the most novel situation for an adult rat or human being is built up of elements which resemble some things he has met many times before. Therefore, if a new question is the stimulus-situation, the responses that we can expect to occur are those which are aroused by stimulus generalization from similar patterns or elements. The strongest will be those conditioned to patterns consisting of some identical and some slightly different elements, as compared with the present pattern, or else those conditioned to single elements. For

example, if no responses have been learned to a combination of 'ants' and 'farming', we can expect responses to occur which are associated with 'ants' plus some other activity, or 'farming' plus some other animal, or with 'ants' or 'farming' alone. This follows from Hull's treatment of stimulus generalization and of patterning (1952, chaps. xii, xix). However, the trains of thought leading out from 'ants' and from 'farming', etc., are likely to be of comparable strength and incompatible. So, here again, conflict may add to the drive-strength.

(3) As these associative processes continue, they are likely to lead, in the absence of other trains of thought, to some which are irrelevant to the motivation to answer the question. This will produce the type of learned conflict to which we have already drawn attention.

(4) Finally, in all probability, concept-patterns will be reached, which are recognized as possible answers. If one is strong and the others are not, then that one will be accepted and learned, and there the process will end. But it may very well be that the subject is faced, either through his own cogitations, or through the intervention of some external agency (as in 'multiple-choice questions'), with a number of possible answers which seem about equally plausible. In that case, conflict between them is to be anticipated. Moreover, if any answer is of such a strength that tendencies to accept or reject it are more or less equal, we shall have another source of conflict, reminiscent of the approach-avoidance conflict. The conflict characteristic of this fourth stage is, of course, particularly prominent in the 'yes-or-no question' (or the 'which—question'). The drive to have the answer will be strongest, according to our expectations, if the tendencies to say 'yes' and 'no' are about equal.

Now, the drive produced in these various ways by conflict can only rightly be called 'curiosity' or a 'drive to know' if it is reduced by the process of knowledge-rehearsal. We must then see how exposure to concepts which are acceptable as the correct answer might lead to conflict-reduction. There are in fact three ways in which this might happen, corresponding to our four cases of conflict as follows:

(1) The answer, by implying that two concepts formerly regarded as incompatible need not be so, may inhibit learned conflict. This would mean that the S–R bond between s_1 and R_{-2}, to use Miller's (1944) notation, would be inhibited by the action of verbal stimuli (Dollard & Miller, 1950). To revert to our example, the subject would cease to find surprising the idea that ants engage in a kind of farming.

(2) and (3) The answer may reduce the conflict derived from irrelevant, generalized trains of thought by evoking a new response-sequence which is strong enough to crowd them out and prevent them from arising. Thus, our subject, having been told something of the activities of harvesting ants, will in future, when confronted with a pattern of concepts combining 'ants' with 'farming', be led off along trains of thought peculiar to harvesting ants and capable of overcoming irrelevant digressions.

(4) The answer may reduce conflict by strengthening one competing response and weakening others, thus reducing the equality between them. This happens when we are made to believe that one of our suspected answers is right and the other is wrong. There is henceforth, therefore, no appreciable competition between them.

Since conflict (F) is an intervening variable, we must ask what variables affect its magnitude. Brown & Farber (1951) postulate that F increases with (1) the absolute strength of the competing tendencies and (2) the equality between them. There is some

experimental evidence for the first of these hypotheses. Sears & Hovland (1941) found that avoidance-avoidance conflicts provoke more blocking when the competing responses are stronger. The assumption that the symptoms of conflict only occur when the conflicting tendencies are about equal is confirmed time after time in Miller's (1951) experiments. Nevertheless, we are obliged to hypothesize that the intensity of the conflict-drive depends also on two more variables, which have been little studied, but which are of prime importance for conflict between thought-processes. These are (3) the number of conflicting response-tendencies and (4) the degree of incompatibility between them. A few remarks on each of these are in order.

First, Miller's treatment of conflict has been confined to cases where only two tendencies are in competition. For that matter, Lewin's, Freud's and Hebb's theories do not consider fully the possibility of there being more than two. Pioneering studies have of necessity to begin with the simplest cases, and there may be special reasons why dual rather than higher-order conflicts are the most frequent. When the theory of higher-order conflicts comes to be attacked it is likely to be found that two or more tendencies among those competing will usually have more in common than others, so that they form an alliance, and the situation resolves itself into what is virtually a dual conflict. It is interesting to note how this happens in Freudian theory. There are three components of the personality—id, ego, superego—but neurotic conflicts take the form *ego* + *superego* vs. *id* or *ego* vs. *id* + *superego* (Fenichel, 1945, p. 132). However, in the higher mental processes, higher-order conflicts may be common. Several trains of thought may be leading symbol-sequences in many incompatible directions at once, just as, in Hull's treatment of learning (e.g. 1930), one stimulus may arouse three or more reaction-potentials at once. It seems reasonable to assume that if the number of conflicting tendencies is increased, all things being equal, the severity of the conflict will increase.

Secondly, both Hull (1952, corollary xiv) and Miller (1944) speak of cases where the incompatibility between response-tendencies is absolute. But, as we have elsewhere mentioned (Berlyne, 1951, p. 144), even with innate reflexes intermediate degrees of incompatibility and partial interference are known. Learned conflict is likely to make even more indispensable the concept of degree of incompatibility, since the $s_1 \rightarrow R_{-2}$ bond may have various strengths. Subjects may, for instance, be surprised to different extents to hear of ants farming.

Our theory may link up with certain threads in contemporary social and developmental psychology. Many writers have been showing (Cantril, 1941, etc.) that one of the most distressing plights for human beings is not to know or to understand a state of affairs, particularly if it is important for their security or contrary to their expectations. One of society's most vital functions is to provide norms and frames of reference for the evaluation of new contingencies. If these are lacking or inapplicable to an unprecedented crisis-situation, people will be prone to accept and spread rumours or to succumb to suggestion. Many of the rumours and fanciful stories accepted by them in this gullible mood are the very reverse of reassuring. Indeed, they often show a tendency to believe in the most alarming prospects they can imagine, so that this desire to have an explanation available does not seem to be reinforced by fear-reduction. But the principal drive behind it may well be the conflict-drive, produced by uncertainty, which, as many wartime phenomena showed, is often more agonizing than realistic anticipation of unpleasantness. When there is a perplexing situation, raising an unanswered question, we predicted from the principles

of behaviour theory that responses due to generalization from similar stimuli would occur. This casts light on the inveterate proclivity, so often observed in children and in primitive peoples, to interpret in terms of familiar phenomena those things that are of moment to them but they have little objective information about, which gives their speculations an 'artificialistic' and 'animistic' stamp (Piaget, 1931). Similarly, when irregular figures are exposed in a tachistoscope, Bartlett (1932) reports that subjects evince an 'effort after meaning' which leads them, as their immediate reaction, to relate the figures to something familiar.

Our theory of curiosity implies that patterns will be most curiosity-arousing at an intermediate stage of familiarity. If they are too unlike anything with which the subject is acquainted, the symbolic response-tendencies aroused will be too few and too feeble to provide much conflict, while too much familiarity will have removed conflict by making the particular combination an expected one. Once again, we have a prediction that accords with the observations of psychologists working in several fields. McDougall (1908) contended that something with an intermediate degree of familiarity is the adequate stimulus for curiosity. Hebb (1949) expresses the belief that the mental processes corresponding to cortical processes ('phase-sequences') will be most rewarding, and therefore most likely to occupy the subject, when the phase-sequences are in the course of being built up. The cell-assemblies and phase-cycles will then be in existence, but they will not yet have been moulded into a firm unity. Freudians (Fenichel, 1945, p. 45) attribute many of the play-activities of children to the discovery that they are now able 'to overcome without fear a situation that formerly would have overwhelmed (them) with anxiety'. But this 'functional pleasure' will be attached to a given activity only during the comparatively short interval between coming to fear it and triumphing over the fear. Piaget (1931, 1945) writes of the apparently useless actions that infants are prone to indulge in repeatedly (primary, secondary and tertiary circular reactions); but each of these actions dominates a child's activity for a brief period while he has discovered the ability to produce the effects in question but has not yet assimilated them to the point where they cease to be interesting.

So far our discussion has concerned itself with postulated and unobservable central processes, which are represented by intervening variables. In order to reveal the empirical content of our theory and the testable predictions that can be derived from it, it will be necessary to state the relations between our intervening variables and their observable antecedents and consequents. This will be done in a forthcoming article in this *Journal*, which will also report some experimental data tending to confirm our hypotheses.

IV. Summary

Human 'epistemic curiosity', to be distinguished from the 'perceptual curiosity' that is found in lower animals as well as in human beings, is defined as a drive reducible by knowledge-rehearsal. An analysis of epistemic curiosity, using a behaviour-theory approach, is presented, its principal features being (1) an account of questions as 'thematic probes' which evoke drive-producing meaning-responses, and (2) the attribution to learned conflict of the curiosity aroused by strange, surprising or puzzling situations or questions.

This article is part of a dissertation submitted to the Faculty of the Graduate School of Yale University in partial fulfilment of the requirements for the degree of Doctor of Philosophy. The author is indebted to Dr C. I. Hovland, thesis adviser, and to Drs I. L. Child and N. E. Miller, members of the thesis committee, for their valuable help. The project was carried out as part of the Yale Communication Research Programme, which is directed by Dr Hovland and financed by a grant from the Rockefeller Foundation.

References

Abraham, K. (1927). Restrictions and transformations of scoptophilia in psychoneurosis: with remarks on analogous phenomena in folk psychology. (In *Selected Papers*. London: Institute of Psychoanalysis, and Hogarth.)

Ach, N. (1910). *Über den Willensakt und das Temperament*. Leipzig: Quelle and Meyer.

Bartlett, F. C. (1932). *Remembering*. Cambridge University Press.

Berlyne, D. E. (1950). Novelty and curiosity as determinants of exploratory behaviour. *Brit. J. Psychol.* li, 68–80.

Berlyne, D. E. (1951). Attention, perception and behaviour theory. *Psychol. Rev.* lviii, 137–46.

Berlyne, D. E. (1954). Knowledge and stimulus response psychology. *Psychol. Rev.* (in the Press).

Bloch, B. & Trager, C. L. (1942). *Outline of Linguistic Analysis*, pp. 52. Linguistic Society of America.

Brown, J. S. & Farber, I. E. (1951). Emotions conceptualized as intervening variables—with suggestions toward a theory of frustration. *Psychol. Bull.* xlviii, 465–95.

Cantril, H. (1941). *The Psychology of Social Movements*. New York: Wiley.

Dollard, J. & Miller, N. E. (1950). *Personality and Psychotherapy*, pp. 119–20. New York: McGraw Hill.

Fenichel, O. (1945). *The Psychoanalytic Theory of Neurosis*. New York: Norton.

Finger, F. W. (1941). Quantitative studies of 'conflict': I. Variations in latency and strength of the rat's response in a discrimination-jumping situation. *J. Comp. Psychol.* xxxi, 97–127.

Freud, S. (1905). *Three contributions to the Theory of Sex*. (In Brill, A. A. (ed.) *The Basic Writings of Sigmund Freud*. New York: Modern Library, 1938.)

Hebb, D. O. (1946). On the nature of fear. *Psychol. Rev.* liii, 259–76.

Hebb, D. O. (1949). *The Organization of Behavior*. New York: Wiley.

Hull, C. L. (1930). Simple trial-and-error learning; a study in psychological theory. *Psychol. Rev.* xxxvii, 241–56.

Hull, C. L. (1943). *Principles of Behavior*. New York: Appleton-Century.

Hull, C. L. (1952). *A Behavior System*. New Haven: Yale University Press.

Koffka, K. (1935). *Principles of Gestalt Psychology*. New York: Harcourt Brace.

Lewin, K. (1917). Die psychische Tätigkeit bei der Hemmung von Willensvorgängen und das Grundgesetz der Assoziation. *Z. Psychol.* lxxvii, 212–47.

Lewin, K. (1922). Das Problem der Willensmessung und das Grundgesetz der Assoziation. *Psychol. Forsch.* i, 65–140, 191–302.

Lewin, K. (1935). *A Dynamic Theory of Personality*. New York: McGraw Hill.

Lowell, E. L. (1952). Experimental studies of the motivational effect of conflict. *Amer. Psychologist*, vii, 253–4 (Abstract).

McDougall, W. (1908). *An Introduction to Social Psychology*. London: Methuen.

Miller, N. E. (1944). Experimental Studies of Conflict. (In Hunt, J. McV. (ed.) *Personality and the Behavior Disorders*. New York: Ronald.)

Miller, N. E. (1951). Comments on theoretical models—illustrated by the development of a theory of conflict behavior. *J. Personal.* xx, 82–100.

Miller, N. E. & Stevenson, S. S. (1936). Agitated behavior of rats during experimental extinction and a curve of spontaneous recovery. *J. Comp. Psychol.* xxi, 205–31.

Montague, W. P. (1936). *The Ways of Knowing*. London: Allen and Unwin.

Morris, C. W. (1946). *Signs, Language and Behavior*. New York: Prentice-Hall.

Mowrer, O. H. (1939). A stimulus-response analysis of anxiety and its role as a reinforcing agent. *Psychol. Rev.* xlvi, 553–65.

Mowrer, O. H. (1950). *Learning Theory and Personality Dynamics*. New York: Ronald.

Mowrer, O. H. & Ullman, A. D. (1945). Time as a determinant in integrative learning. *Psychol. Rev.* lii, 61–90.

Mowrer, O. H. & Viek, P. (1948). An experimental analogue of fear from a sense of helplessness. *J. Abn. (Soc.) Psychol.* xliii, 193–200.

Osgood, C. E. (1952). The nature and measurement of meaning. *Psychol. Bull.* xlix, 197–237.

Piaget, J. (1931). Children's philosophies. (In Murchison, C., *Handbook of Child Psychology.* Worcester, Mass.: Clark University Press.)

Piaget, J. (1945). *La formation du symbole chez l'enfant.* Neuchâtel: Delachaux and Niestlé. (*Play, Dreams and Imitation in Childhood.* New York: Norton, 1951).

Piaget, J. (1947). *La psychologie de l'intelligence.* Paris: Colin. (*The Psychology of Intelligence.* London: Routledge, 1950.)

Sears, R. R. & Hovland, C. I. (1941). Experiments on motor conflicts. I. Determination of mode of resolution by comparing strengths of conflicting responses. *J. Exp. Psychol.* xxviii, 280–6.

Skinner, B. F. (1953). *Science and Human Behavior.* New York: Macmillan.

Skinner, B. F. (1947). *Verbal Behavior.* William James Lectures, Harvard University, 1947. (To be published.)

Ullman, A. D. (1951). The experimental production and analysis of a 'compulsive eating symptom' in rats. *J. Comp. Physiol. Psychol.* xliv, 575–81.

Wertheimer, M. (1945). *Productive Thinking.* New York: Harper.

(*Manuscript received* 14 *December* 1953)

A NOTE ON ASHANTI NAMES AND THEIR RELATIONSHIP TO PERSONALITY

By G. JAHODA

University College of the Gold Coast

I. *Introduction* (p. 192). II. *Background of traditional beliefs* (pp. 193). III. *Methods used* (pp. 193–194). IV. *Results and conclusion* (pp. 194–195). *References* (p. 195).

I. Introduction

The present paper originated with a discussion at Kumasi, Ashanti, in which mainly teachers and welfare officers took part. The question at issue was the development of character in children. Several of the people present maintained that the day of the week on which a person is born determines what type of character will emerge. The writer, who had not long been in Africa at that time, was politely sceptical. But one headmaster, whose previous contributions had proved him to be an intelligent and well-informed man, claimed that he had put this matter to the test. It appeared that he had consulted his records of corporal punishment and had found that those born on Wednesdays were by far the most frequently in trouble; this accords with popular beliefs.

The writer was sufficiently impressed by this statement to consider the possibility, not of course that the day of birth determines character, but that prevailing beliefs in this matter may do so, especially when they are held by educated as well as by illiterate Ashanti. Over half the people present at the meeting were firmly convinced that differences of the kind indicated are a fact; the remainder, whilst knowing about these beliefs, were doubtful; only two out of sixteen denounced them as superstitions.

The view that the expectations of other people influence the development of a person's character has been held by many psychologists. For instance McDougall (1936) wrote:

> (the child) gets his idea of his self in large part by accepting the ideas of himself that he finds expressed by those about him. The process is well illustrated by the case of the unfortunate child who is constantly scolded and told that he is a naughty boy. Under these conditions the normal child very soon accepts these oft-repeated suggestions, learns to regard himself as a naughty boy, and plays the part thus assigned to him. Similarly, if he finds himself constantly regarded as clever, or irresistibly charming, or in any other light, he can hardly fail to regard himself in the same way, and the idea of his self moulded in this way by his social environment affects his conduct accordingly.

Others have followed McDougall, for example, Murphy, Murphy & Newcomb (1937), but there appears to be no supporting empirical evidence. The reason is perhaps to be sought in the fact that parents' and other people's attitudes towards children vary greatly and irregularly in Western Society, thereby making the task of tracing specific effects a hopelessly complicated one. In the present case an element of uniformity is introduced by the existence of socially determined beliefs about certain defined categories of children. It therefore becomes possible to use a quantitative approach, provided satisfactory criteria of conduct or personality can be found.

II. Background of traditional beliefs

Before describing the methods used in the investigation, a brief outline of beliefs about names is necessary. Every child in Ashanti and other *Akan* areas receives a name from the day on which he is born. The name thus derived is called *kradin* or soul name; it refers to the day on which the soul is 'washed' or purified (cf. Rattray, 1927). These names are listed in Table 1.

Table 1. *Ashanti 'day names'*

Day of the week	Boys	Girls
Sunday	*Kwasi*	*Akosua*
Monday	*Kwadwo*	*Adwoa*
Tuesday	*Kwabena*	*Abenaa*
Wednesday	*Kwaku*	*Akua*
Thursday	*Yaw*	*Yaa*
Friday	*Kofi*	*Afua*
Saturday	*Kwame*	*Amma*

There is no evidence that any significance is attached to girls' names. With boys the beliefs about character traits corresponding to each 'day name' show some regional variation, but there is widespread consensus about two of them: a *Kwadwo* is supposed to be quiet, retiring and peaceful; by contrast a *Kwaku* is held to be quick-tempered, aggressive and a trouble maker. Similar conceptions are implicit in the appellations relating to these days. That for Monday is *Awo*: 'It is the day of the moon or the vesper which is "cool"; the day of peace' (Danquah, 1945). Wednesday's address is *Awuku*; the root *wu* in this means 'to die'; *di awu* is 'to commit a murder' (Christaller, 1933). Furthermore, a phrase is current about *Kwaku babone*, the bad Wednesday boy; one informant told me that during the War people in his village used to talk about *Kwaku* Hitler.

It should perhaps be mentioned that day names merely indicate that a particular type of *kra* has entered the body; such a *kra* produces a disposition towards certain kinds of behaviour, but the disposition may to some extent be counteracted and does not amount to a fixed destiny.

The naming customs described are based upon a magico-religious system of ancestor-worship which still affects Ashanti life in numerous ways, in spite of widespread, although often superficial, conversion to Christianity. Literates, who are usually Christians (education having been until recent years largely in the hands of missions) represent a minority of about 6% of the population. Practically all of them continue to give their children day names and very few are entirely divorced from the older ideas.

III. Methods used

It is clear, therefore, that these beliefs are powerfully entrenched in tradition, and likely to produce a set of expectations concerning the ways in which a *Kwadwo* or a *Kwaku* will behave. A direct check of this hypothesis was unfortunately impracticable for someone who is not a fulltime field worker; it would have involved prolonged and intensive study of differential attitudes, both overt and covert, towards those born on the various days of the week. More superficial inquiry did reveal that phrases like 'A *Kwaku* naturally would do this kind of thing' are quite commonly used.

Turning from process to product, namely the resulting personality differences, other differences are encountered. It is unlikely that such differences could be readily detected, and suitable criteria established, with available personality 'tests' or techniques; in any case these are as yet unproven in Africa.

A rather crude test, similar to the one used by the headmaster, is however possible, which has the advantage of providing fairly clear-cut behavioural criteria. It consists in examining the Juvenile Court records for the region to ascertain the frequency of conviction and type of delinquency committed by boys born on different days of the week. This task was accordingly undertaken, without any sanguine hopes of achieving significant results.

As a preliminary step the names of all boys in ten Kumasi schools were obtained. This was done to ensure that no general bias was present in the distribution of *kradin*; for it is conceivable, for instance, that in the case of births occurring around dawn the 'good' rather than the 'bad' day might be chosen; it has to be understood of course that there may be a variety of reasons why a particular day should be thus regarded.

Then the delinquency records were consulted for the period between September 1948 (when the Juvenile Court started working) and June 1953. Boys belonging to tribes other than Ashanti were excluded. Offences were divided into those against the person (e.g. assault, fighting, causing wound), against property (e.g. stealing, receiving, fraud) and 'other offences' (e.g. traffic offences, unlawful assembly, wandering). Recidivists were classified according to their first offence. In spite of repeated careful checking aliases may have been a source of error; but there is no reason to suppose that their incidence would affect some day names more than others; furthermore, there were indications that they occurred only rarely, so that their total effect was almost certainly negligible.

IV. Results and conclusion

The heading 'Other' in Table 2 refers to those cases where no information about *kradin* was available; this happened more often with the delinquents, when existing data had to be used. School-teachers were specially requested not to omit them.

Table 2. *Distribution of names: ordinary schoolboys and delinquents*

Ashanti names ...	*Kwasi*	*Kwadwo*	*Kwabena*	*Kwaku*	*Yaw*	*Kofi*	*Kwame*	Other*	Total
Corresponding days ...	Sunday	Monday	Tuesday	Wednesday	Thursday	Friday	Saturday		
Schoolboys	179	157	151	164	163	179	188	73	1254
Delinquents	47	23	42	45	49	63	63	114	446

Test of significance D.F. $=6$, $\chi^2 = 15{\cdot}75$, $P = 0{\cdot}02$.

* The category 'Other' was omitted in computing χ^2.

It will be seen that there is a significant difference between the two distributions, which is mainly accounted for by the small proportion of *Kwadwos* among the delinquents: this alone contributes a proportionate square of 10·13 out of a total χ^2 of 15·75. *Kwadwos* thus appear to live up to their reputation of being quiet and well-behaved as compared with boys born on other days.

On the other hand, the supposedly difficult *Kwakus* (numbering 45) come close to the expected figure (47·43) as far as all types of delinquency are concerned. However, if one considers only offences against the person, which are presumably the most direct expres-

sion of an aggressive disposition, the picture fits again. For *kradin* other than *Kwaku* the absolute number of such offences ranges from 2 (*Kwadwo*) to 8 (*Yaw*), with a mean of 4·8; for *Kwaku* the number is 12 and this, as is apparent from Table 3, represents a significantly higher proportion than that found in all the rest taken together.

Table 3. *Offences against the person*

Type of name	Offences against the person	All other offences	All offences
All except *Kwaku* (including 'other')	43	358	401
Kwaku only	12	33	45
All delinquents	55	391	446

Test of significance D.F. = 1, $\chi^2 = 9{\cdot}52$, $P = 0{\cdot}002$.

The results here presented are consistent with the hypothesis that Ashanti beliefs about a connexion between personality characteristics and day of birth may be effective in selectively enhancing certain traits which otherwise may have remained latent. The evidence is of course indirect and as such not conclusive. Nevertheless, the correspondence appears too striking to be easily dismissed. It is probable that for many Ashanti there is some truth in the saying *nomen est omen*.

The writer wishes to acknowledge with thanks the assistance of the Regional Director of Education, Ashanti, and the Probation Officer, Kumasi. He is greatly indebted to his colleague Mr Joseph Kwabena Nketia, Research Fellow in African Studies, without whose contribution this study could not have been undertaken.

References

Christaller, J. G. (1933). *Dictionary of the Asante and Fante Language.* Basel: Evangelical Missionary Society.

Danquah, J. B. (1945). *The Gold Coast Akan.* London: Lutterworth Press.

McDougall, W. (1936). *An Introduction to Social Psychology*, 25th ed., p. 160. London: Methuen and Co.

Murphy, G., Murphy, L. B. & Newcomb, T. M. (1937). *Experimental Social Psychology*, p. 211. New York: Harper and brothers.

Rattray, R. S. (1927). *Religion and Art in Ashanti.* Oxford: Clarendon Press.

(*Manuscript received* 23 *February* 1954)

EVIDENCE OF IMPROVEMENT IN POETIC JUDGEMENT

By J. N. BRITTON

Institute of Education, University of London

I. *Introduction* (p. 196). II. *Aim* (p. 196). III. *Procedure* (pp. 196–198): (i) *Preparing the test* (pp. 196–197); (ii) *Setting the test* (p. 197); (iii) *Methods of analysis* (pp. 197–198). IV. *Results* (pp. 198–205): (i) *Performance of the eighteen experts* (pp. 198–199); (ii) *Critical comment upon the experts' performance* (p. 199); (iii) *Performance of the whole sample of* 221 *raters* (pp. 199–200); (iv) *Analysis by groups of raters* (p. 200); (v) *Critical consideration of four points arising* (pp. 200–202); (vi) *The test repeated* (pp. 202–203); (vii) *A comparison of average orders* (pp. 204–205); (viii) *The convergence of preference* (p. 205). V. *Summary and conclusions* (pp. 206–207). *References* (p. 207). *Appendix: details of test items and examples of counterfeit poems* (pp. 207–208).

I. Introduction

Experience seems to suggest that changes of opinion concerning works of art include among them a number that are the effect of maturing judgement or 'improvement' in taste. That is to say that a number of these changes are such that they would be likely to move in one direction (say to promote item A above item B), unlikely to move in the opposite direction (to promote B above A), and extremely unlikely to be reversed, once made in what, by hypothesis, is the right direction. If this is the case it should be possible, among a sufficient number of observations, to discern a tendency for opinions to converge upon certain items at the expense of certain others. This hypothesis is tested here in the field of poetry.

In a previous experiment the writer (Britton, 1953), following Eysenck (1940), found clear evidence of the importance in poetic preference of the distinction between simple poetry and complex. 'Determiners' were prepared for this factor, as also for Eysenck's second bipolar factor—that distinguishing between poetry of restrained feeling and poetry of 'great emotionality'—by pooling ratings carried out by experts for the criteria concerned. The material used consisted of thirty-one short poems by modern writers, selected as being of approximately equal esteem in the opinions of experts.

II. Aim

The purposes of the present experiment were: (i) to employ a 'test' to measure the response of a number of persons to 'goodness' and 'badness' in poetry; (ii) to repeat the test after an interval in order to try out the hypothesis that there would be a convergence of preference upon the good poems (i.e. an 'improvement' in poetic judgement); and (iii) to compare the influence of the distinction between good and bad poetry with the influence of the two bipolar factors referred to above.

III. Procedure

(i) *Preparing the test*

There is an obvious danger here that doubt may be cast upon the validity of the distinction between good and bad poetry as reflected in the examples chosen. Can the badness and goodness of a test poem be established as more than a matter of the experimenter's opinion? The pooled opinions of experts provide satisfactory measures of fairly

complex criteria, but so simple a means of setting up objective aesthetic value itself would be unacceptable.

A solution was found in this case by avoiding any kind of hierarchy in the material and using instead a discrete, dichotomous arrangement. No attempt was made to distinguish degrees of goodness among the good poems: eight poems were taken from thirty-one used in a previous experiment, and it was assumed that in each the poet had something to communicate: that he had no intention in writing other than such as he hoped a reader would discern. Nor was any attempt made to distinguish degrees of badness: the bad poems were counterfeit or spurious, concocted by the experimenter with the deliberate intention of deceiving readers. They were made to look like poetry, but, *since they have nothing to communicate*, they are not.

The true poems were selected so that two poems represented each of the four poles of Eysenck's factors. Seven false poems were prepared in rough conformity with the same four categories. The following diagram illustrates the composition of the test: examples of the false poems and details of the true poems are given in the Appendix.

	True poem nos.	False poem nos.
Simple	5 and 12	1 and 3
Complex	2 and 11	4 and 14
Abandoned	6 and 10	7 and 13
Restrained	9 and 15	8

(ii) *Setting the test*

Subjects were asked to arrange the poems in the order of their preference and, wherever possible, add comments giving reasons for their assessment. Two hundred and twenty-one subjects took the test on the first occasion, eighteen of them being experts (English honours graduates, teachers of poetry), the remainder non-experts (including ten post-graduate English students, ten English honours undergraduates, forty training college students, eighteen Services personnel, and 110 sixth-form pupils, of whom sixty-four were taking arts courses and forty-six science courses).

A hundred and twenty of these took the test a second time after an interval of 4–6 months. These included ten postgraduate students, four undergraduates, forty training college students and sixty-eight sixth-form pupils, forty-two in arts and twenty-six in science.

Raters on the second occasion were asked to give their *present* reactions, 'which might conceivably differ from your previous ones'.

(iii) *Methods of analysis*

Preference ratings were allotted scores roughly in accordance with normal distribution as follows:

Score	+4	+3	+2	+1	0	−1	−2	−3	−4
Frequency	1	1	2	2	3	2	2	1	1

The significance of poem totals or category totals, above and below the mean of zero, was measured by Fisher's adaptation of Student's test for use with small samples (Fisher, 1936, pp. 125–6). The significance of changes from first to second rating was also calculated in this way, using differences between members of each pair of scores as themselves scores.

Analysis of variance was employed to discover whether the true/false distinction could be seen to influence raters, and how its effect compared with that of the bipolar factors.

An analysis of the variance between and within poems was first carried out, and this was followed by an analysis of the variance between and within the categories 'true' and 'false', and between and within the four bipolar categories—these analyses being carried out on the whole series of fifteen poems. Significance was measured by comparing the variance between categories, in turn, with the variance remaining after that attributable to both sets of categories had been deducted.

When, to quote an example, the ratings of the eighteen experts were analysed in this way it was found that the variance between true and false poem groups was significant (in favour of true) at the 0·1% level, and the variance attributable to the four bipolar categories was also significant, but at the 1% level.

More interesting light was shed upon the bipolar categories by analysing separately the group of poems classified as simple or complex, and the group classified as abandoned or restrained. Variance between true and false poems was also calculated in each case. The eighteen experts, for instance, showed a preference for the complex poems which was significant at the 5% level, some apparent preference for the true poems which did not reach significance level, and, in the other group of poems, a preference at the 0·1% level both for the restrained and for the true poems.

This analysis was felt, however, to be inadequate as a means of assessing the influence of the bipolar factors. In studying the effect of the true/false distinction the main concern was with the effect in one direction: the power of attraction of true poetry. In assessing such a power, the choice of true poems by some raters would be rightly offset by the failure of others to prefer them. But in the case of the bipolar factors, by their very bipolarity, the attraction of, for example, complexity for some readers and the attraction of its opposite for others should count alike as part of the influence of the bipolar factor. A group of raters which was equally divided in this respect, however strongly its members were attracted and repelled by poems in the category concerned, would yield a non-significant result. Clearly this masking effect made the method unsuitable.

The idea was considered of reversing the direction of some scores so that all differences appeared in the same direction, and an adequate representation of the *extent* of the variance thus given. It was decided, however, that a similar indication of its extent could be given together with indication of *direction* if two analyses were carried out: one for those raters who preferred one category in the pair and a second for those who preferred the other. This was in fact carried out wherever two groups of opinion existed.

Finally, similar analyses (in two groups of raters where necessary) were carried out upon the scores for true poems only. It will be recalled that true poems were allotted to bipolar categories on the evidence of the experts, whereas false poems were not.

These methods were employed both to analyse the total variance for all raters, and also to analyse the variance of selected groups of raters: significances were sought, in terms of which the preferences of these groups might be characterized and distinguished.

IV. Results

(i) *Performance of the eighteen experts*

The experts showed a highly significant preference for the true poems as a class (mean true-poem score per rater, +4·056; S.E. 0·962). Three true poems (nos. 6 and 10, abandoned, and no. 12, simple) had negative totals, and three false poems (nos. 4 and 14, complex,

and no. 1, simple) had positive totals, though none of these reached the level of significance.

By analysis of variance it was found that the variance attributable to the true/false distinction, in favour of true, was very highly significant, and so also was that attributable to the four bipolar factors. Further analysis showed that the discrimination in favour of true poems operated principally among the abandoned/restrained poems, and was accompanied by a general and highly significant preference for the restrained poems. Among the simple/complex poems, on the other hand, there was no significant preference for the true poems, but both simple and complex poems had their adherents among the raters, preference reaching the highly significant level. This one-sidedness of the abandon/restraint distinction and two-sidedness of the simple/complex distinction tended to operate with all groups of raters.

(ii) *Critical comment upon the experts' performance*

A number of experts failed to achieve a positive true-poem total, and this raises an important issue. Is there any justification for expecting the experts to prefer the true poems (of whatever kind) to the false (of whatever kind)? The rating, after all, was one of liking and not of value.

There can, in the long run, be only one answer. A poem is not comprehended until some experience has been communicated to the reader by its words. Until it is comprehended it may be liked (or disliked) for many reasons, all of them secondary: the sound of the words may please, the ideas stated may be acceptable. If, finally, it becomes clear that the 'poem' has no experience to communicate, it can only be rejected as worthless or counterfeit.

A time factor, however, enters into the present situation. It takes time to discover a hoax, as it takes time to comprehend a poem: both remain 'potential' until they have made, or finally failed to make, some kind of impact upon the reader. It is suggested that wherever a poem did make an impact upon a reader it was either positively endorsed or positively rejected. Now the nature of the false poems is such that they would be less likely to make an impact than the true ones: 'making an impact' is a way of describing the process of synthesis as it begins to work in the reader's consciousness; and in such a process nothing succeeds like success. A negative result—no impact, no synthesis, no experience, no poem—would be likely to take longer to arrive at. It is, as it were, a process of elimination. It follows that the false poems (if they succeeded in deceiving at all) would tend to remain *sub judice* longer than the true poems.

(iii) *Performance of the whole sample of* 221 *raters*

Whereas poem totals in the experts' case showed no significant results in the wrong direction, the totals given by the whole sample show significant disapproval of true two poems (no. 2, complex, and no. 10, abandoned), and significant approval of four false poems (nos. 4 and 14, complex, no. 8, restrained, and no. 13, abandoned).

Analysis of variance of the whole sample demonstrated the importance of the bipolar categories as controlling preference (though the influence of the 'abandoned' category seemed to work in a negative direction usually): and the complete ineffectiveness of the true/false distinction. In the whole table summarizing results of the various analyses

carried out, only one entry occurs in the true/false column: this indicates that 29% of the raters showed a significant preference for abandoned poems together with an only slightly less significant preference for the false poems in this group.

(iv) *Analysis by groups of raters*

Raters were grouped in three ways:

A. By maturity (i) Experts and students
(ii) Second- and third-year sixth forms
(iii) First-year sixth forms

B. By 'faculty' (i) Arts sixth forms
(ii) Science sixth forms

C. By sex (i) Girls
(ii) Boys

Of these seven subcategories:

(*a*) Only the experts and students showed in the analysis of variance any preference for the true poems as a class. The boys showed a significant preference, and the science students a highly significant preference, for the false poems.

(*b*) The division by maturity demonstrated very clearly the gradual change from preferring simple poems, in the earlier stages, to preferring complex poems in the later.

(*c*) The arts students distributed their favours fairly evenly among the bipolar categories (significant preferences, 56% pro simple, 44% pro complex: 38% pro abandoned, 62% pro restrained), whereas the science students showed the most one-sided distribution (74% pro simple, 26%—but not reaching significance—pro complex: 26% pro abandoned, 74% pro restrained).

(*d*) Girls' results differed very little indeed from those of boys.

(v) *Critical consideration of four points arising*

From a detailed study of these results, four matters are raised for special comment.

First, it was found that in all groups except the experts and students a preference for true simple poems existed side by side with a liking for false complex poems. (A large interaction term was found in carrying out a full analysis of variance of ratings for these poems.)

There is no need to explain at length the average rater's rejection of false simple poems: they were easily seen to be what they were—meaningless. Discrimination in favour of false complex poems is, however, a more difficult matter.

There is ample evidence, from group scores and from occasional comments, that all but the poorest raters tended to suspend judgement upon a poem they did not understand. This would have the effect of placing such a poem below those understood and liked and above those understood (or seen through) and disliked: in other words, on average, near the middle of the scale.

Other considerations, however, resulted in discrimination between the true and the false complex poems, the former being demoted from the neutral position. Study of comments shows that raters frequently gave lukewarm approval, or mild or trivial disapproval to the false complex poems (referring often to poetical language, familiar allusions, vague air of mystery, etc.): whereas many indignant attacks were launched

against the tone or manner or apparent attitude of the writer in the true complex poems. It will be noted that in both cases (in default of knowledge of the poems' full meaning) it is incidental qualities or superficial appearances that arouse these reactions.

It is suggested that the originality itself, the bold intentions, of the true complex poems, brought about this resentment. For the poet to have his way with a reader in a poem of this kind would have meant a more *disturbing* experience for him than would be the case with a less complex or less original poem. When for any reason a reader is unable to respond to a strong stimulus of this sort a sense of frustration, irritation, resentment, would appear to be a more usual reaction than mere indifference.

The second matter is of a similar nature, and, once again, is probably the phenomenon underlying a fairly large interaction term found in analysing variance in some groups' ratings of the abandoned/restrained poems. There seems to have been among the less experienced raters a tendency to discriminate against true abandoned poems and yet distribute preferences fairly evenly among the remaining three categories (false abandoned, true restrained, false restrained). Why should a consistent preference for restrained poems be accompanied by a willingness to approve false abandoned poems?

Of the false abandoned poems, one (no. 7) dealt with love in its familiar form of love between the sexes: the other (no. 13) was a sentimental treatment of nature-and-friendship, a pathetic fallacy in the vein of Ella Wheeler Wilcox. The two true abandoned poems were both love poems in the accepted sense.

It is quite clearly a rejection of love poems and a strong preference for the nature poem that principally brought about the result under consideration. Two groups, however—the science forms and the boys—also prefer the false love poem to either of the true ones. One explanation will be offered for both tendencies.

It may be said of conventional symbols and stock images that, as 'objective correlatives', to use Eliot's phrase, they are useful and reliable.[1] No normal person can completely disguise from himself the fact that he is capable of comparatively strong feelings, and, however he may wish it to appear to others, he is at times willing or more than willing to respond to people, objects or experiences which excite such feelings and in some measure satisfy them. The stock situation in life and art, because it is already familiar, enables him to do so within strict bounds: it acts rather as a rein than as a spur. The unfamiliar stimulus of an original and powerful work of the imagination is always a threat to these self-imposed bounds and will often, it is suggested, be rejected for this very reason. This may account for the curious fact that habitual readers of a kind of writing that is regarded as 'sentimental' (or even 'sloppy') by experienced critics, themselves describe any original expression of strong feelings as sentimental or sloppy. The terms indicate equally a rejection, but the reasons in each case are, as has been suggested, very different.

If a tendency to cling to stock symbols and situations is a self-protective measure, it may well be particularly resorted to during phases of emotional instability; as, for example, during the period of adolescence.

It is part of a cultural education to teach a child to supersede the stock situation in what he reads, in order that art may so come to enrich experience. If education succeeds

[1] 'The only way of expressing emotion in the form of art is by finding an "objective correlative"; in other words a set of objects, a situation, a chain of events which shall be the formula of that *particular* emotion, such that when the external facts which must terminate in sensory experience are given, the emotion is immediately evoked' (Eliot, 1934, p. 61).

at all in this respect, the stock response (see Richards, 1935, pp. 15–16) would be more prevalent among those whose cultural training has been for any reason neglected. And this, unfortunately, one might expect to find more often in the science forms, especially among boys. Especially among boys, because for one thing it is traditional in this country to regard free expression of feeling as effeminate. The tradition may even be founded upon a real difference between the sexes, or may result from social divergences in the role of man and woman. Once again, if tradition and inclination combine to suggest caution to the boy, a part of his response is likely to be a prejudice in favour of the stock situation and the conventional symbol at the expense of genuine feeling new-made into poetry.

The third point is that among the undergraduates as a group, and affecting also a number of training college students, there was found a strong vein of antipathy to simple poetry, whether true or false, and a corresponding preference for complex poetry, true or false.

This effect will not surprise readers who have dealings with undergraduates, though the sharpness with which it appears in the figures and the intensity of some of the comments might. It is probably explained in part by the fact that one of the processes involved in becoming an experienced reader is that of acquiring the skill to comprehend more and more difficult writing; and that pride in this maturing process may be accompanied by impatience of any work which does not call forth the skill concerned. Or, more simply, it may be a matter of regarding as childish anything that a child might understand—a judgement appropriate perhaps to the transitional stage of adolescence.

Finally, comments gave abundant evidence of the operation of prejudice. It must be borne in mind that the process of judgement necessarily consists in referring data observed to standards already held, the fruits of past experience. It is when the fruits of past experience obtrude to hamper the observation of data that they may be called prejudices.

The commonest form of prejudice met was that of looking only for the recurrence of old and familiar pleasures: approving recollections of what is already thoroughly acceptable without asking for any new light to be shed upon it. 'I am an admirer of nature poetry and this poem reminds me of Keats's poem *To Autumn*', was one girl's comment upon poem no. 13.

There were, on the other hand, many extraordinarily precise preconceptions: as that 'never' in the last line of poem no. 12 should have been 'ne'er'; or that poems should have so many lines to a stanza and rhyme in such and such a way.

The strongest distortion seemed to come from a preconceived notion as to what a poet's *attitude* should have been towards his theme. Failure to evince the attitude expected resulted often in a charge of insincerity. There were widespread doubts as to Edward Thomas's sincerity in poem no. 12.

(vi) *The test repeated*

The test was set a second time in order to discover whether, among the changes observed, there was any tendency for preferences to converge upon the true poems. There was. Individual true-poem totals increased to give a difference of mean of 1·250 compared with S.E. of difference of means of 0·326: and a similar result was obtained by taking individual differences between first and second true-poem/totals for each rater and comparing the mean of this distribution with its S.E. ($\bar{x} = 1{\cdot}250$; S.E. $= 0{\cdot}331$). Changes

of opinion in the group as a whole, therefore, concerning the poems of the test showed an improvement, a convergence upon the true poems at the expense of the false.

Table 1 represents graphically the changes made in assessment of each poem by the whole group of 120 raters. Significant variations from the mean are indicated, for each occasion, in the first and third columns: significance of difference of means, representing changes of opinion (increases being regarded as positive), form the middle column. It will be seen that the two true complex poems (nos. 2 and 11) and one true restrained poem (no. 9) show significant increases, while one true simple poem (no. 12) deteriorates significantly. Among the false poems there is only one significant change (at the lower level, under 5% probability)—the deterioration of poem no. 7 (abandoned).

Table 1. *Results of significance of means, and of difference of means, tests by poems (for 120 raters)*

(Single sign = under 5% probability; double sign = under 1% probability.)

		First rating	Difference	Second rating
True poems				
Simple	No. 5	(+ +)	.	(+ +)
	No. 12	.	− −	(− −)
Complex	No. 2	(− −)	+ +	.
	No. 11	.	+ +	(+ +)
Abandoned	No. 6	.	.	(−)
	No. 10	(− −)	.	(− −)
Restrained	No 9	(+ +)	+ +	(+ +)
	No. 15	(+ +)	.	(+ +)
False poems				
Simple	No. 1	(− −)	.	(− −)
	No. 3	(− −)	.	(− −)
Complex	No. 4	(+ +)	.	(+ +)
	No. 14	(+ +)	.	(+ +)
Abandoned	No. 7	(− −)	−	(− −)
	No. 13	(+ +)	.	.
Restrained	No. 8	(+ +)	.	.

It is suggested that the process of first suspending judgement on the complex poems and later, in the case of the true complex poems, coming down on the side of approval, would explain the entries in the table for poems 2, 11, 4 and 14.

The deterioration of poem no. 12 provides a useful warning: that it is a general tendency among the raters cannot be denied, and yet it is not in the direction looked for. The simplicity, the regular verse form and, to some degree, the sentiments expressed earned for this poem from a number of raters the comment 'Georgian'—a term that already has in it the opprobrium of one generation for the generation that immediately precedes it. The element of fashion, it is suggested, will often lead to sweeping changes of opinion in poetry: but, viewed broadly enough, these will be temporary and subservient to those ampler movements, slower but more certain, by which opinion would be found to converge upon genuine poetry.

It is gratifying to notice the comparative lack of change relating to false poems. Changes have to be caused, and the impact of the true poem would be more likely to incur change than the failure of the false poem (with no experience to communicate) to make an impact on the reader. Changes in position of the false poems are more likely therefore to be by gradual displacement.

(vii) *A comparison of average orders*

Candidates who took the test both times were divided into groups similar to those used to divide the whole sample of 221 raters, with the exception that on this second occasion there were two categories only in the division by maturity (second and third-year sixth forms: first-year sixth forms). Although significant changes in group poem-totals were fully examined, the more rewarding method of comparing performances proved to be by considering average orders of poems. Three standards were used as bases of comparison:

The average order of the whole sample of 221 raters.
The average order of the eighteen experts.
A skeleton 'order' in which true poems were given priority over false poems and all other differences ignored.

Comparisons with the first and second of these (converted into scores as described in §III (iii) above) were made by product-moment correlation: a similar index for comparison with the third was obtained by correlating the set of scores to be measured with a hypothetical set in which the numerical scores were a repetition of its own, but the signs were made positive for all true poems and negative for all false. The three measures intercorrelated thus:

	18 experts	*T*/*F* order
221 raters	0·43	0·06
18 experts	.	0·91

an indication of the variation in influence of the true/false distinction as between experts and the whole sample.

The average orders for 120 raters at first and second ratings reflect, by these standards, the improvement already shown to exist:

Correlation with ...	221 raters	18 experts	*T*/*F* order
First rating	0·91	0·36	0·23
Second rating	0·84	0·77	0·43

Breaking these results down into terms of groups of raters gives Table 2 below.

Table 2. *Characteristics of group average orders at first and second ratings*

Correlation with ...	221 raters		18 experts		*T*/*F* order	
Rating ...	1st	2nd	1st	2nd	1st	2nd
Second and third year	0·61	0·57	0·54	0·76	0·46	0·71
First year	0·87	0·93	0·06	0·63	−0·40	0·34
Arts forms	0·70	0·74	0·09	0·50	0·03	0·46
Science forms	0·66	0·80	−0·27	0·13	−0·57	−0·26
Boys	0·61	0·60	−0·13	0·23	0·06	0·06
Girls	0·77	0·83	0·10	0·60	0·00	0·26

Slight changes in the first (double) column indicate the fairly stable degree of general agreement or conformity among the raters, on either occasion or by any classification. Within this, however, lies considerable room for movement towards the more informed opinions of the experts (second column) and the objective true/false standard (third column). The superior ability of the second- and third-year group is clearly shown, but it is left to the first-year group to reveal the greatest improvement from first to second rating: a fact which suggests that changes are due at least in some degree to changes in

taste or in ability on the part of the raters. Had they been due wholly to the effects of a second contact with the poems the effects would surely have been more marked with the more able and experienced readers (provided of course there was room for such improvement).

The science forms stand out for their tendency to prefer the false poems (though they show considerable improvement), and the boys are remarkable for lack of change. The division by sexes yields in fact two groups which show comparatively little improvement in the third column of the table: division of substantially the same sample into other groupings shows more change, particularly the division by maturity. This may suggest that the contributions of the two sexes in some way complement each other. (Boys' order correlated with girls', 0·37: first-year group with second- and third-year group, 0·74.)

It may be added that a similar relationship in this respect seems to hold among individual experts making up the group of eighteen. When correlated in turn with the true/false order the highest coefficient gained by any expert (and by one only) is 0·91. The average order of all experts nevertheless achieves the same result, 0·91: suggesting that their individual ratings pool to give something more like a composite order than a simple average.

(viii) *The convergence of preference*

It is satisfactory to have found a convergence of preference upon the true poems (though with exceptions), but the question may be asked, what caused this convergence? Does the change indicate an improvement in individual poetic taste during 6 months? Or the fact that a second attempt at a poem, after an interval, takes a reader a stage nearer to a true appreciation? One shred of evidence was offered in §(vii) above, but obviously the evidence of the present experiment, in which the *same* poems were presented twice, cannot provide conclusive answer, and it might be more prudent to suggest that the two explanations need not be regarded as entirely separate. If repeated readings of a poem over an interval of time could be shown to lead to a truer appreciation of that poem, it would have to be demonstrated that such an experience did not also tend to induce a more favourable approach to *other* poems before it could be claimed that the two questions were, in normal circumstances, independent.

Time spent reading poetry seems to be the core of the matter. Changes of opinion with regard to poetry on the part of people who never read poetry (for what the subject is worth) could clearly have no connexion with the training effect of repeated readings. The training given to the raters taking part in this experiment would be so diverse that no generalization can be made about it except to say that in all but a minority of cases some time would be spent by the candidates in reading and considering poetry. (The science forms, who would be likely to spend less time reading poetry, certainly showed a poorer result.) In general the results suggest that, provided the habit of reading poetry was sufficiently present for some poems at least to make an impact, the study of these poems, repeated after an interval, was likely to give rise to changes of opinion in the right direction.

It is suggested that the best teachers of poetry have always worked upon the assumption that time spent in reading good poetry will, in spite of initial setbacks, contribute towards the formation of good poetic judgement.

V. Summary and conclusions

A battery of eight short poems by modern writers and seven counterfeit 'poems', prepared for the occasion, was presented to 221 raters, eighteen of whom were accounted experts. The eight true poems represented four categories based upon two bipolar factors extracted in an experiment by Eysenck (1940): namely, simple and complex poetry; and poetry of restrained and abandoned feelings. Raters arranged the poems in the order of their preference and gave comments in explanation. One hundred and twenty of the original raters carried out the process a second time after an interval of 4–6 months.

Preference orders were converted into scores roughly representing the frequencies of normal distribution. Results were analysed by use of Student's test as applied by Fisher (1936) to the means of small samples: and by the method of analysis of variance.

The experts were found to show a significant preference for true poems (particularly in the categories 'abandoned' and 'restrained') and significant preferences for restrained poems and either for complex poems (in a majority of cases) or for simple poems (in a minority of cases). The whole sample of 221 raters showed no discrimination whatever in favour of true poems as a class, but were significantly influenced by all four of the bipolar categories. Raters were divided into groups in various ways and results further analysed.

The difference between simple and complex poetry was found to be an issue in poetic preference with all groups of raters. Notably, the less experienced preferred the simple poems, the more experienced the complex poems, though the distinction seemed less influential with the experts themselves. The distinction between abandoned and restrained poetry was found to operate one-sidedly, in favour of restraint, a result which may well reflect the revolt in recent years against romanticism in poetry.

Categories derived by analysis of poetic judgements, categories of actual opinion, are obviously of very great importance in the study and in the teaching of poetry. It is perhaps easy to make the mistake of going further and regarding them as in some way prescriptive—a role for which they would be even less fitted than the categories laid down from time to time by experienced critics. For the teacher they will be a starting-point, rather than any kind of destination.

It was inferred from ratings and comments by raters that the more experienced reader tends to respond to a poem either by committing himself fairly positively (both in endorsement and in rejection) or by suspending judgement upon it. It is the sign of the reader least experienced in poetry that he rejects what he cannot comprehend simply on the grounds that he cannot comprehend it.

The primary obstacle to poetic judgement was found to lie in the intervention, between the reader and the expression of the poet, of preconceived ideas, attitudes and sentiments. It is suggested that stock responses and other forms of prejudice may sometimes be adopted out of instinctive caution: a protective measure, so to speak, against the new experience where it is of an emotional kind; and a refuge instead in the familiar emotional situation preconceived.

The re-test revealed a significant increase in true-poem totals as a whole, that is a convergence of preference upon the true poems. The primary condition for its occurrence would seem to be that poetry should be read and returned to.

There seemed some evidence that average orders of preference ratings might in some

cases be regarded as 'composite orders', a pooling of complementary opinions. As an observer's angle of view may be unique and his view yet complementary to other views of the same object from other angles, so greater objectivity might sometimes be gained by pooling poetic judgements than would appear possible by any consideration of the overlap of individual views.

The writer gratefully acknowledges the help of Prof. P. E. Vernon with this research.

References

Britton, J. N. (1953). A factor analysis of poetic preferences.*

Eliot, T. S. (1934). *Elizabethan Essays.* London: Faber.

Eysenck, H. J. (1940). Some factors in the appreciation of poetry, and their relation to temperamental qualities. *Character Pers.* IX, 160–7.

Fisher, R. A. (1936). *Statistical Methods for Research Workers,* 6 ed. Edinburgh: Oliver and Boyd.

Richards, I. A. (1935). *Practical Criticism.* London: Kegan Paul.

Appendix

Details of test items

(Counterfeit poems are referred to in brackets)

No.

1 (Emblem)
2 The Youth with the Red Gold Hair—Edith Sitwell
3 (Word out of Season)
4 (The Sea)
5 Bright Clouds—Edward Thomas
6 Francesca—Ezra Pound
7 (Faithful unto Death)
8 (If there is a no-man's land)
9 The Villagers and Death—Robert Graves
10 I dreamed that I stood—W. B. Yeats
11 I tell you the generations of man—Archibald Macleish
12 In Memoriam, Easter 1915—Edward Thomas
13 (Autumn)
14 (Casualty List)
15 September Fires—Herbert Read

Examples of Counterfeit Poems

(3) Word out of Season

Yours was always the voice I heard
However many spoke beside.
Amidst the clattering tongues one word
From you would bring me to your side.

When you were with me all you said
I treasured in my heart, until,
Goodbye being over, in my head
Your precious words were ringing still.

I took your words to bed with me:
Now nothing else is mine to keep—
The voice is gone—the mocking words
Destroy my peace, deny me sleep.

* Copy may be had on loan from the Technical Information and Documents Unit, Department of Scientific and Industrial Research.

(13)

Autumn

Autumn has draped the boughs in gold and gossamer;
The woods are hers: her mists are silver in the air,
And on the mist, what bitter scent of burning years!
What requiem for rose and daffodil, what tears
For leafy bowers soon naked and sunny petals shed!
We walk the dusk and speak of summer's glories dead.

And yet the woods are lovely still! No requiem
Need steal their ageing beauty, nor disfigure them
With bitter thoughts. Silver glides the swollen stream
Beneath the branchy elms: the moulting tree-tops seem
As lovely in the autumn light as ever dappled sun and shade
That corruscated leaf and limb upon the grassy glade.

Love too from spring to autumn moves, and when the sun
That shone on golden lad and lass at last has run
Its fiercest noon-day flight, and first approaching night
Spreads lengthened shadows on the path, the mellow light
Of true affection, flooding as the sun's late rays,
Shall shine in tears and laughter down love's autumnal days.

(14)

Casualty-list

Proud Pharoah, long from flesh, preserves his dust
In golden pomp: no sacrilegious touch
Disturbs his dreams; a thousand years he must
Alone have waited on the perfumed flesh.
Pale silken queens their lonely vigil keep
Bedecked for flattery that none shall pay;
Only the bone remembers: none could weep
For love of lovely dust or moving clay.

Whom the gods love die swift: no burial
Bestows their bones with runes and lingering grief,
'And some there be have no memorial',
So quick and clean is death. O fate be brief!
All flesh is grass and fiercely burn the fires of war:
No rites of grief shall stay them at the opening door!

(*Manuscript received* 1 *December* 1953)

A SEX DIFFERENCE IN PREFERENCES FOR SHAPES

By W. A. McELROY

Psychology Department, University of Sydney

I. *Introduction* (pp. 209–211). II. *Test administration and test results* (pp. 211–213). III. *Interpretation of the sex differences* (pp. 213–216). IV. *Summary* (p. 216). *References* (p. 216).

I. Introduction

A feature of many experiments concerned with visual aesthetic preferences has been the variety of sources from which the subjects have reported aesthetic enjoyment. In addition to other elements, Valentine (1950) has suggested that 'the general preference for curves over straight lines may be something very fundamental and connected with the appeal of curves in the human form'. For our everyday aesthetic preferences, Thouless (1947) has stated a possible corollary of psychoanalytic theory in the following terms:

> It cannot be doubted that the pleasure taken in the appearance of the human form is the result of the appeal that it makes to the tendency with a more powerful content of pleasure than any other—the sex instinct. The form most strongly appreciated as beautiful is almost invariably that of the opposite sex....Those who appreciate the widespread symbolism of sex will not doubt that sexual associations lie at the bottom of the beauty perceived in many other objects. At least, we must recognize that much of the pleasure in works of art may be due to their unrecognized appeal to the sex emotions.

From biological principles, it is stated here that a basic sex difference in aesthetic appreciation is to be found in the male preference for the female form, and the female preference for the male form. Considering the role played by symbolism in mental life, it is alleged that a modified but derived sex difference must also exist with respect to suitable symbolic representations of male and female forms. This possible corollary suggests, therefore, that perception by the male of any suitable symbolic representation of the female form may be accompanied in consciousness by a surplus of pleasurable affect denied to the female, with a converse process applying to perception by the female of any suitable representation of the male form.

The female form differs essentially from the male in its curved aspects, especially with respect to the full development of the breasts, and it appears likely that this quality of the female form may be suitably represented symbolically by simple curved diagrams. In addition, the psychoanalysts have maintained that pointed forms are typical male symbols (Freud, 1911).

Considering these aspects of the symbolism theory, and its possible corollary, a twelve-item aesthetics test was constructed. This test is exactly reproduced in Fig. 1. Each item consists of a pair of line drawings, one labelled '*A*' and the other '*B*'. For each item, one of the drawings was selected on account of its curved features and the other was selected because of its possible symbolic phallic properties. For each of the twelve items shown in Fig. 1 an asterisk has been placed against the label of the line drawing intended to represent disguised aspects of the female form. (These asterisks were not present in the test proper). Six items, irregularly distributed throughout the test, had curved drawings first; the other six items showed curved drawings last as the *B* number.

That the shapes chosen for the test have in some way either 'masculine' or 'feminine' significance is supported in part by the work of other investigators[1] and their premises. In experiments reported by Franck (1946) round forms are regarded as feminine designs, while masculine designs consisted of elongated forms. Alschuler & Hattwick (1947), in observation of the aesthetic expression of children, have reported that straight lines or

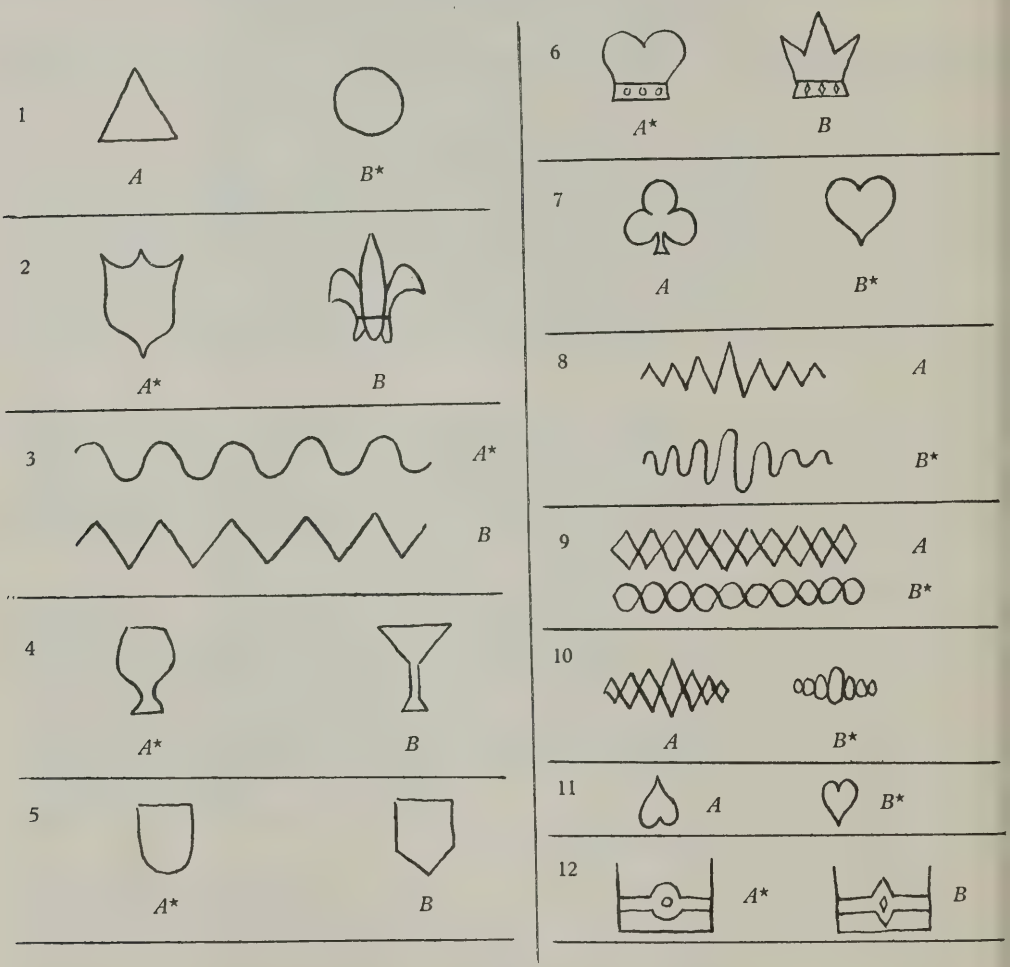


Fig 1.

elongated figures are characteristic products of aggressive moods of the more 'masculine' children. They also observe that children with negative feelings towards their mothers tended to destroy rounded forms in their drawings. Supporting evidence also comes from Krout (1950), who found that her subjects responded to a semicircle as a 'female' form, and responded to a straight line as a 'male' form.

[1] Item 1 of the aesthetic appreciation test reported here consists of a triangle and a circle. This pair of stimuli were used by Dr Florence Kluckhohn in a study of phantasy content in relation to pattern of culture (personal communication). It was found that the subjects—U.S. students—tended towards a phantasy stereotype of describing the triangle and circle as 'a man and his girl friend'. An interesting feature of the study was the tendency of the subjects towards personalization of figures.

Let us suppose that a number of subjects of both sexes are instructed to state on record sheets individual preferences for *A* or *B* for each of the twelve items of the test. It is possible to predict from the corollary previously stated that sex differences will exist in the responses of the group, on account of the postulated 'surplus of affect' being distributed in opposite directions for the male and female groups. It is to be noticed that this corollary does not justify the prediction that the number of responses other than male preferences for 'female' shapes and female preferences for 'male' shapes will not differ significantly from zero, since many other factors such as the symmetry and evenness of the drawings must be expected to influence test results. It should also be noted that identification responses, i.e. Lipps's (1905) empathic responses, might also be a mechanism of preference. If this were the primary basis for preference, a sex difference might be expected to exist in the opposite direction to the prediction stated above, so that a greater proportion of female than male subjects would record preferences for 'female' shapes.

II. Test administration and test results

It was considered undesirable to apply the test to an experimental group of adult subjects, particularly University students. It seemed probable that the content of the test would be more disguised for younger subjects. The actual experimental group consisted of 779 children from three different Scottish schools, comprising 380 boys and 399 girls between the ages of 9 and 16. Three hundred and seventeen subjects (148 boys and 169 girls) were between the ages of 9 and 12 and formed the older half of a Scottish Elementary school; 33 subjects (17 boys and 16 girls) were aged 11–13 and formed a preparatory class about to enter a Scottish Secondary school; and 429 subjects (215 boys and 214 girls) were between the ages of 11 and 16 and formed the first four years of a Scottish Senior Secondary school. (The schools were non-feepaying schools in the suburbs of an industrial city.)

The test items were duplicated upon sheets headed 'Picture Test'. These sheets were distributed to the subjects, who were tested in their classrooms in groups of approximately forty subjects. In most cases each group contained members of both sexes. Blank paper was distributed for the records of preferences, and the subjects were asked to place name and age upon this paper, in addition to marking the numbers one to twelve in the margin. The subjects were then informed that they were to try a 'Picture Test', and were asked to look at test item number one (Triangle and Circle). The instructions given to all groups were as follows:

> Look at number one on the sheet with the pictures. Opposite this number are two pictures—a triangle and a circle. The triangle is called *A* and the circle is called *B*. What you have to do is to decide which you like better. If you like the triangle better than the circle, then put *A* opposite number one on the sheet with your name on it. If you like the circle better than the triangle, then put *B* opposite number one on your paper. When you have done this, continue with the other items in the same way. Does anyone not understand what they have to do?...Remember that in this test there are no right or wrong answers. We want to know what pictures *you* like best.

During the test, children were discouraged from discussing items or answers with one another. Answer sheets were collected from individual subjects as soon as they had been completed. The frequencies of the responses to the twelve test items for 380 male subjects and 399 female subjects are shown in Table 1. Also shown are the proportions of the male and female groups recording preferences for those diagrams intended as disguised

'female' configurations, and for those intended as disguised 'male' configurations. An inspection of these data for each item shows that either (*a*) a greater proportion of male than female subjects recorded preferences for 'female' configurations, or (*b*) a greater proportion of female than male subjects recorded preferences for 'male' configurations. The disparity in aesthetic judgements between the groups could have been produced by either possibilities or both.

Table 1

	Males		Females		Proportions of preferences: 'Female' shapes		Proportions of preferences: 'Male' shapes				
Item	*A*	*B*	*A*	*B*	Male	Female	Male	Female	χ^2	*c*	r_p
1	297	83	355	44	0·218	0·110	0·782	0·890	12·08	3·48	0·15
2	88	292	54	345	0·232	0·135	0·768	0·865	9·72	3·12	0·12
3	210	170	155	244	0·553	0·388	0·447	0·612	21·36	4·62	0·16
4	214	166	204	195	0·563	0·511	0·437	0·489	1·95	1·31	0·05
5	148	232	78	321	0·389	0·195	0·611	0·805	32·45	5·29	0·21
6	212	168	155	244	0·558	0·388	0·442	0·612	22·71	4·41	0·17
7	206	174	264	135	0·458	0·338	0·542	0·662	12·08	3·48	0·12
8	241	139	309	90	0·366	0·226	0·634	0·774	17·57	4·19	0·15
9	281	99	350	49	0·261	0·123	0·739	0·877	18·18	4·27	0·18
10	215	165	265	134	0·434	0·336	0·566	0·664	8·42	2·90	0·08
11	119	261	163	236	0·687	0·591	0·313	0·409	6·11	2·47	0·09
12	190	190	162	237	0·500	0·406	0·500	0·594	7·22	2·69	0·09

$\chi^2 = 169{\cdot}85$; $r_p = 0{\cdot}14$; $c_{mf} = 13{\cdot}03$.

For each item the significance of this disparity was first estimated by applying a χ^2 test of significance between the frequency of the responses which appeared to be in agreement with the psychoanalytic corollary (i.e. the frequency of male preferences for 'female' shapes plus female preferences for 'male' shapes) and the frequency of the responses which appeared to be in disagreement (i.e. the frequency of male preferences for 'male' shapes plus female preferences for 'female' shapes). In each case the square root of the resultant χ^2 was extracted to give an exact measure of the critical ratio of the difference between the proportions of the sex groups recording preferences for 'female' configurations, or between the proportions of the sex groups recording preferences for 'male' configurations. The critical ratio *c* gives the magnitude of the tested disparity in terms of a normal deviation with unit standard measure, and the value for each item is entered in Table 1. An inspection of these values shows that the sex difference is fully significant at the 0·01 level for all items except nos. 4 and 11, although the latter item provides a sex difference significant at the 0·05 level. It will be seen that pooling the results for all items gives a resultant χ^2 of 169·85. The square root of this value provides an estimate of the critical ratio of the difference between the proportions of the sex groups recording preferences for 'female' shapes—or between the proportions of the sex groups recording preferences for 'male' shapes—over the experiment as a whole, and this critical ratio c_{mf} has the value 13·03. The level of significance is therefore extremely high, and the deviation is in the direction predicted from the possible corollary of psychoanalytic theory. Considering the direction of the deviation, it is apparent that identification responses have not provided the principal basis for preference, although it is likely that they have contributed towards the total experimental results.

From an analysis of the data given in the table, the mean male test 'score' is 5·22

preferences for disguised 'female' configurations or 6·78 preferences for disguised 'male' configurations. The estimated standard deviation of the male 'scores' is 1·71, and the standard error of the mean is 0·493. The mean female test 'score' is 3·75 preferences for disguised 'female' configurations or 8·25 preferences for disguised 'male' configurations. The estimated standard deviation of the female 'scores' is 1·60, and the standard error of the mean is 0·462.

It will be seen that although the difference between the sex groups tends to be fully significant for most items, the correlation between sex and type of preference is not itself large. A fourfold point correlation r_p is given for each item in Table 1, and this varies from 0·05 for item no. 4 to 0·21 for item no. 5. Over the twelve items the average correlation is 0·14.

To examine the relationship between age and preference for shapes,[1] a simple dichotomous classification of the test data was made according to whether the subjects had stated an age level below 12 years, or had stated age as 12 years or above. The number of preferences for 'female' shapes is now given for these two age groups, and the critical ratio of the difference between the older and younger divisions of the sex groups with respect to the number of preferences for 'female' shapes is also given (as estimated from the root of a χ^2 test of significance):

	Male		Female	
	No. subjects	No. preferences	No. subjects	No. preferences
Below 12 years	161	745	182	739
12 years or above	219	1238	217	757
	c_d=4·16		c_d= −2·96	

From these data it will be seen that a significant rise in male preferences for 'female' configurations has occurred between the younger and older male groups. A corresponding smaller but significant decrease in female preferences for these configurations has also been recorded.

III. Interpretations of the sex differences

It was stated earlier that it is possible to entertain some alternative hypotheses other than psychoanalytic or biodynamic ones which might account for some aspects of the results obtained. These hypotheses will be considered first.

(*a*) An inspection of the test reproduced in Fig. 1 shows that the drawings varied in their accuracy. Since rectilinear drawings are easier to draw than curvilinear ones, it is possible that the curvilinear drawings contain a greater proportion of inaccuracies than the rectilinear ones. It can be further maintained that girls are more critical of minor inaccuracies than boys, hence their preferences. A close inspection of Fig. 1 suggests that

[1] In a paper entitled 'Experimental Tests of some Aspects of Psychoanalytic Hypotheses' read (by Dr R. W. Pickford in the absence of the present writer) at the Annual General Meeting of the British Psychological Society in April 1951, it was stated that age appeared to make little difference to the reported sex difference in preferences for shapes for the age levels studied. (In that paper the number of male preferences for 'female' shapes was added to the number of female preferences for 'male' shapes and classed as responses in 'Agreement' with psychoanalytic hypotheses. Other responses were classed in the 'Disagreement' category). This statement was based upon two empirical observations. First, evidence of the sex difference was found at all the age levels studied. Secondly, a comparison between the extremes of the age groups had revealed apparent consistency in the magnitude of the sex difference as given by the ratio of Agreement to Disagreement responses. It is clear from the more appropriate analysis of the test results given above, that a relationship with age does exist.

this hypothesis might certainly apply to test items nos. 1 and 4, where the circle[1] and the wine glass with the curved profile can be seen to contain more inaccuracies than their corresponding 'male' shapes. Yet it should be noted that these items, where the 'female' shapes contain probably greatest inaccuracies relative to the 'male', are not the ones which produce the greatest deviations in test results. In fact item no. 4, with the 'female' shape perhaps poorest in accuracy relative to the 'male' shape, provided the smallest deviation of all the items. We note, further, that some of the intended 'male' configurations involved in some cases the drawings of as much curvilinear outline as the corresponding 'female' configurations, viz. items nos. 2, 7 and 11. Yet these items provided wide deviations in test results. In items nos. 3, 5, 6, 8 and 12, the inaccuracies do not appear to be confined to any one type of diagram rather than the other type, whereas item no. 9 suggests that the rectilinear outline is, if anything, more poorly drawn than the curvilinear one. Whether this hypothesis could account for the relationship between age and preference also appears doubtful. Nevertheless, it is possible that the factors suggested by the hypothesis may have contributed towards test results as a whole.

(*b*) Some of the shapes are likely to have specific associations for the subjects. For example, with item no. 2 (*A*, shield, *B*, fleur de lys) it is possible that *A* might suggest to the boys a shield or coat-of arms, whereas outline *B* might be more familiar to girls as a pattern found on materials for curtains, etc. In this way the associations of the boys and the girls might be different for the same shape. Prof. Sir Cyril Burt asked several subjects the reasons for their preferences after these had been obtained. Some of the associations are given below:

Item No. 2. The majority (of the boys) undoubtedly preferred the shield—usually because it was a *shield*, i.e. a symbol of warlike adventure....Two of the girls said that the fleur de lys reminded them of Joan of Arc, and added that was why they liked it. Only one referred to the shield as a *shield*, and she added that that was why she *disliked* it.

Item No. 3. Several girls said they would have preferred the wavy line if it had been better drawn. Two of those who preferred the zigzag line said it would be much easier as a pattern for stitching.

Item No. 4. The boys liked the idea of drinking wine out of the big rounded wine-glass. The girls tended to prefer the other wine glass, which two of them called a 'champagne glass'; and they obviously thought champagne glasses were much more interesting than the old Georgian pattern.

It is certainly true that some features of the cultural background which foster adventurous boys and less adventurous girls may have contributed towards the test results obtained for item no. 2 in the way the above associations suggest. Yet whether this would account for all of the deviation obtained with item no. 2 seems open to question. Other features of the cultural background might suggest that boys should not prefer the shield. For example, the fact that the fleur de lys is shown on the Boy Scout's badge could be used as evidence of this. Whether similar special features of the cultural background other than those associated directly with sex (as with the drinking glasses suggested by the associations for item no. 4) could account for all of the deviations for all of the items, especially the less meaningful ones, is again open to doubt.

In considering reasons given by the subjects for their preferences, the question arises as to how far it is possible to advance *valid* reasons for aesthetic preferences, and also how far reasons advanced are sole determinants of the preferences. No doubt it is at times possible to advance valid reasons for aesthetic preferences, but for many aesthetic

[1] Several female subjects studied by Prof. Burt commented on the fact that the circle was not *perfectly* circular, and added that if it had been they would probably have preferred it.

judgements this would not necessarily be the case. Likes and dislikes, for example, that are based upon conditioned emotional reactions, are unlikely to be accompanied by conscious awareness of the true reasons for aversions and preferences. It is possible that associations of the kind reported by the subjects studied by Prof. Burt have contributed towards test results. Yet the possibility of over-determination of object choice might also be considered. Thus if any boys tended to select the shield through operation of some unconscious determinant they might be even more inclined to select the shield since it is a *shield*—a justifiable choice of boys.

(*c*) Since the sex difference was predicted from a suggested corollary of psychoanalytic theory, it is at once apparent that the test results can be interpreted as supporting the psychoanalytic symbolism theory. By this theory, as stated by Jones (1918), a powerful affective and conative process, i.e. some behaviour tendency invested with libido, which is held in a state of psychical repression, and in consequence is denied entry to consciousness and is not permitted free external expression, may be symbolized by some idea in consciousness, owing to the existence of a process of unconscious identification. In this way the occurrence of a conscious idea or percept can be a partial means of gratification of a repressed idea, and hence such an occurrence may be accompanied by an affective state derived not as the result of the occurrence of the conscious idea or percept as such, but as the result of the partial expression of the unconscious repressed idea. It is clear, however, that the experiment cannot be regarded as forming a complete experimental analogue of the symbolism theory, since the content of the symbolism appears to be less extensive than the theory as stated by Freud. It certainly appears unlikely that *all* of the types of objects that Freud (1911) believed to be typical female symbols which occur in dreams (e.g. stoves, rooms, and wardrobes) would be perceived by the male in accompaniment with a surplus of aesthetic enjoyment; or that tree-stumps, umbrellas, or nail-files would necessarily provide additional aesthetic pleasure to the percepts of females.[1] It should be noted, however, that the sex difference could have been predicted from certain well-established principles of learning, without involving psychoanalytic concepts.

(*d*) The well-established principle of 'stimulus generalization' could have been used as the basis of prediction of sex differences in responses to the test items. Miller (1935) has reported generalization of fear responses in animals from one food cup to a highly similar drinking cup. Similarly, Masserman (1946) has described generalized fear of 'all food boxes and associated configurations' after cats had been placed in a conflict situation centred around one particular food box. From this principle, with reference to 'associated configurations of the male and female form', we could have predicted sex differences in preferences. If this principle were used as an explanatory one, then it might be possible to account for the significant rise in male preferences for female-associated configurations through a tendency for the primary stimulus to be reinforced with greater pleasurable affect.[2] For the female group, however, a similar explanatory principle might appear unlikely. It would be possible to account for the relationship with age in terms of generalization of fear responses, since it is around 12 years and later that problems of

[1] Evidence from clinical psychologists does suggest, however, that many unusual objects can provide aesthetic enjoyment for individual persons. A. A. Brill (1914), for example, quotes an interesting case of a lady who collected coloured plates of mushrooms, and suggests that this has symbolic significance.

[2] In psychoanalytic terms, this relationship with age could be explained as occurring through an increase in repressed affect.

female adolescent development are likely to occur. These possible explanations of the relationship between age and type of preference are, of course, speculative. It is further possible that any valence that the shapes had for the children could have been assimilated unconsciously from the valences placed upon different shapes by adults, and that the relationship with age might reflect the increase in assimilation of post-adolescent valences.

Whatever interpretation is accepted, it is possible that the sex difference is culturally delimited.

IV. Summary

This paper reports the existence of a sex difference in preferences for shapes as shown by the results of an aesthetics test carried out with Scottish schoolchildren. The sex difference was predicted from a possible corollary of psychoanalytic theory, so that it is possible to consider the test results as supporting Freudian principles. Some alternative hypotheses, however, can be advanced to account for the results obtained. It is further possible that the sex difference cannot be explained in terms of any one hypotheses considered alone. Whatever interpretation is accepted, the writer is at present concerned only incidentally with describing the construction of the aesthetics test and method of prediction of the sex difference, and is primarily concerned with reporting the test results.

The writer is indebted to Prof. Sir Cyril Burt for many detailed comments upon the empirical data reported in this paper. The writer has called freely upon these comments in preparation of the text. He is also indebted to Prof. Burt for supplying the reasons given for aesthetic preferences made by some subjects studied by him using the test described here.

References

Alschuler, R. H. & Hattwick, L. W. (1947). *Painting and Personality.* Chicago: University Chicago Press.

Brill, A. A. (1914). *Psychoanalysis*, p. 234. Philadelphia: Saunders.

Franck, K. (1946). Preferences for sex symbols and their personality correlates. *Genet. Psychol. Monogr.* xxxiii, 73.

Freud, S. (1911). *Die Traumdeutung*, 3rd ed., p. 211. Leipzig and Wien: Deuticke.

Jones, E. (1918). *Papers on Psychoanalysis.* Chapter: 'The theory of symbolism.' London: Baillière, Tindall and Cox.

Krout, J. (1950). Symbol elaboration test. *Psychol. Monogr.* no. 310, lx, no. 4.

Lipps, T. (1905). *Psychologische Studien.* Leipzig.

Masserman, J. (1946). *Principles of Dynamic Psychiatry*, p. 109. Philadelphia: Saunders.

Miller, N. E. (1935). A reply to 'sign gestalt or conditioned reflex?' *Psychol. Rev.* xlii, 80.

Thouless, R. H. (1947). *General and Social Psychology*, 2nd ed., p. 452. London: University Tutorial Press.

Valentine, C. W. (1950). *Psychology and Its Bearing on Education*, p. 439. London: Methuen.

(*Manuscript received* 17 *November* 1953)

A NEW TACHISTOSCOPE AND CYCLING TIMER

By W. C. R. WITHERS

Psychology Department, Institute of Psychiatry, University of London

I. *Tachistoscope* (pp. 217–218). II. *Timer* (pp. 218–219). III. *Summary* (p. 219). *References* (p. 219).

I. Tachistoscope

The need for an improved tachistoscope and timer arose out of an 'apparent movement' study conducted by Brenner (1953) in this department. In this study a modified Dodge tachistoscope similar to the one described by Merryman & Allen (1953), was used. In operation the first figure was seen directly through an eyepiece and half-silvered mirror; the second figure being seen by reflexion in the half-silvered mirror. This necessitated complete illumination of the cards from one side, so that not only the stimulus figure was illuminated, but also the inside of the box. Further, if the pattern on the cards was at all complex, correct alinement of both cards became extremely difficult to achieve and the apparent movement occurred in two directions simultaneously.

To overcome these disadvantages, a direct viewing tachistoscope (Fig. 1) was devised.

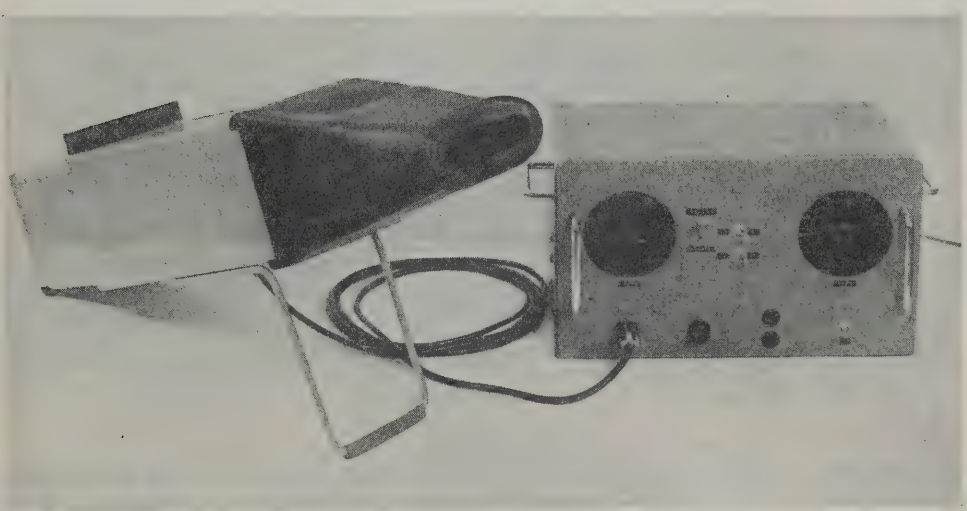


Fig. 1. New tachistoscope and electronic timer.

This tachistoscope consists of a 6 in. square metal tube 15 in. in length, painted black inside; a rubber viewing mask at one end, 11 in. long (ex Radar) fits closely round the subject's eyes. The other end fits over a square sectionalized lamp housing containing four standard 220 V. 5 W. neon lamps, each mounted to illuminate a quarter of the box. A slit in the body of the tachistoscope in front of and above the lamp housing, allows cards of black bakelite to be inserted. Stimulus figures are cut out of these cards. A metal

mask in front of the cards holds them firmly against the lamp housing, so that light can neither be seen round the edges of the cards, nor escape from one section of the lamp housing to another. The bakelite cards themselves have white typing paper pasted on the reverse side to give even illumination of the figures. The top two lamps are on at all times, and the stationary part of the figure is cut out in front of them. To obtain apparent movement, the lower two lamps are switched on in turn by an electronic timer. To allow the lamps to be connected in different combinations, a tag panel is mounted on the back of the lamp housing. The apparatus is fitted on a stand to present the eyepiece at eye level to the seated viewer.

This tachistoscope has the advantage that only the figures are seen, and there are no alinement difficulties.

Other applications

By altering the lamp connexions, other experiments can be carried out. For example, flicker-fusion tests using one lamp only: or apparent rotary motion can be obtained by adding a further metal section between the top two lamps.

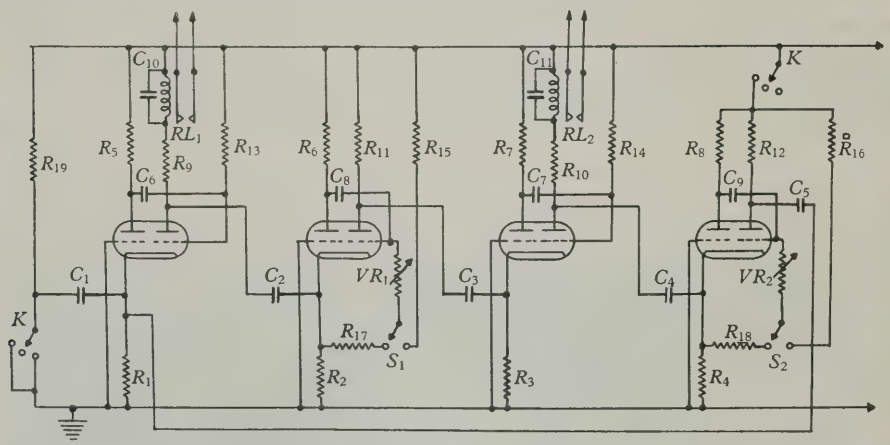


Fig. 2. Circuit diagram of timer. *List of components.* Resistors: R_1, R_2, R_3, R_4, 2·4 kΩ ±10%; R_5, R_6, R_7, R_8, 100kΩ; R_9, R_{10}, 6·8 kΩ; R_{11}, R_{12}, 10 kΩ; R_{13}, R_{14}, 1·5 MΩ; R_{15}, R_{16}, 390 kΩ; R_{17}, R_{18}, 330 kΩ; R_{19}, 10 MΩ; VR_1, VR_2, 5 MΩ (linear). Capacitors: C_1, C_2, C_3, C_4, C_5, 0·001 μF.; C_6, C_7, 0·1 μF.; C_8, C_9, 0·15 μF. Relays: RL_1, RL_2, 3·4 kΩ, Siemens high-speed relays. Switches: S_1, S_2, single pole 2-way (range switches); K, double pole 3-way key switch. Valves: All 6 SN7, each half matched within 15%.

II. Timer

The timer for the first tachistoscope was an electronic one in which a multivibrator circuit drove round a uniselector which connected the lamps for the required period. This was a simple arrangement, but had the disadvantage of producing clicks giving auditory cues to the subject; also this timer had no facilities for cycling the stimulus figure.

A new timer was developed to overcome these defects, the circuit diagram of which is presented in (Fig. 2).

Operation

When the experimenter presses down the operate key (K) the first 'flip flop' (2) turns on the first of the two lower lamps for a fixed period of 50 msec. (controlled by $R_{13}C_6$). On flopping back, a sharp negative pulse is injected into the cathode of the second 'flip flop',

flipping it over for a period determined by the setting of (VR_1). When this 'flip flop' reverts it operates the third 'flip flop' and the second lamp for 50 msec. ($R_{14}C_7$).

For cycling, the operate key is set in the upward position, connecting a further stage, the fourth 'flip flop'. This causes the whole cycle of operations to be repeated after an interval determined by the setting of VR_2. With the key (K) in the centre position, the timer is inoperative.

Other applications

Several applications are possible. For example, a similar timer has been arranged by the writer to give a conditioned and overlapping unconditioned stimulus, for use in conditioned reflex experiments. The overlap is achieved by connecting C_2 from the second 'flip flop' directly to the operate key (K), the extent of the overlap then being determined by VR_1.

Circuit notes

The relays for the neon lamps are Siemens high-speed relays, and are mounted on sponge rubber in a box of acoustic boarding. With the timer in its case, no operating sounds can be detected at about 3 ft. away.

The capacitors C_{10} and C_{11}, shown in the circuit diagram (Fig. 2), are to prevent high oscillatory voltages from being generated and passed to the next stage.

The coupling from anode to cathode of succeeding 'flip flops' (which the writer believes to be an original arrangement) obviated the need for phase-inverting stages.

The initiating network ($R_{19}C_1$) is arranged so as to produce only negative pulses.

The duration of the flashes is fixed at 50 msec. The intervals between them are variable from 18 msec. to 3 sec. (calibrated with a 'Chronotron' Timer).

Power supplies are: h.t. 150 V. neon stabilized, and 220 V. d.c. for the neon lamps obtained directly from the mains via a metal rectifier and smoothing capacitor.

III. Summary

The construction and working of a new tachistoscope and new electronic timer are described. Directly viewed cut-out figures are used in this tachistoscope. The timer is a silent, electronic model giving either cyclic or single exposures.

The writer is indebted to May Brenner and W. D. Furneaux, of this department, for their many helpful suggestions relating to the design of the tachistoscope and timer.

References

Brenner, May (1953). Unpublished Ph.D. Thesis, University of London.

Dickinson, C. J. (1950). *Electrophysiological Technique*, p. 43. London: Electronic Engineering.

Merryman, J. G. & Allen, H. E., (1953). An improved electronic tachistoscope. *Amer. J. Psychol.* lxvi, 110.

(*Manuscript received* 11 *September* 1953)

CORRESPONDENCE

The Editor
British Journal of Psychology

Dear Sir,

In the current volume of the *Annual Review of Psychology* there is a paragraph by Masserman, Gross & Pechtel (1954) commenting on my article on experimental neuroses that recently appeared in this *Journal* (Wolpe, 1952*a*). The paragraph not only ignores all the major conclusions of the article, but in addition contains several inaccurate and misleading statements to which attention is drawn below.

The paragraph opens with this sentence: 'Wolpe reported his observations on 12 cats subjected to grid shocks with or without learned feeding responses, and in general confirmed the results of similar experiments by previous workers.' This statement glosses over the crucial point that *because* neuroses were produced by shocks *without* the animal having previously learned feeding responses in the experimental situation, motivational conflict in Masserman's sense could not have been essential to their production. Masserman (1943) and other workers (e.g. Dimmick, Ludlow & Whiteman, 1939) had earlier assumed that conflict was essential, because feeding responses were involved in all their experiments, and controls without feeding were not done. Since my experiments contradicted this assumption there is no justification for the suggestion of unreasonableness on my part conveyed in Masserman, Gross & Pechtel's second sentence—'Nevertheless he objects to Masserman's concepts of motivational conflict...'; for such an objection is precisely what was substantiated.

The same sentence continues with an assertion that I propose that 'experimental neuroses are induced (*a*) by "ambivalent stimulations" arising from difficult discriminations, delay before reinforcement, or rapid reversals, and (*b*) "noxious stimuli" which interrupt learned responses'. Anyone would gather from this that items (*a*) and (*b*) comprise my *explanation* of experimental neuroses, whereas, in fact, they are merely some of the many experimental situations I enumerated as productive of neuroses. (Noxious stimuli are effective *whether or not* they interrupt feeding responses.) Actually, I tried to show that all the various effective situations are anxiety-evoking and that experimental neuroses are due to the conditioning of powerful anxiety responses to previously 'indifferent' stimuli.

In the light of the foregoing it is necessary to reject also Masserman, Gross & Pechtel's final remark that my formulations 'eventuate in about the same procedures both for inducing and treating the neurotic deviations'. When new facts change theories, new procedures eventuate. In the present context this has already happened. Both experimentally (Wolpe, 1952*a*) and clinically (Wolpe, 1952*b*, 1954) treatment procedures have emerged that are very different from those generated by Masserman's theory.

Yours faithfully,

J. Wolpe

Department of Psychiatry
University of the Witwatersrand

References

Dimmick, F. L., Ludlow, N. & Whiteman, A. (1939). A study of 'experimental neurosis' in cats. *J. Comp. Psychol.* xxviii, 39–43.

Masserman, J. H. (1943). *Behaviour and Neurosis.* Chicago: University of Chicago Press.

Masserman, J. H., Gross, Z. & Pechtel, C. (1954). Abnormalities of behavior. *Ann. Rev. Psychol.* v, 263–80.

Wolpe, J. (1952*a*). Experimental neuroses as learned behaviour. *Brit. J. Psychol.* xliii, 243–68.

Wolpe, J. (1952 *b*). Objective psychotherapy of the neuroses. *S. Afr. Med. J.* xxvi, 825–9.

Wolpe, J. (1954). Reciprocal inhibition as the main basis of psychotherapeutic effects. (*Arch. Neur. and Psych.* To be published.)

The Editor

British Journal of Psychology

Dear Sir,

The publications listed below[1] will show that during the past fifteen years my colleagues and I have noted and specifically controlled the phenomena reported by Dr Wolpe. I shall, therefore, not further dilute the literature by replying in detail to Dr Wolpe's comparisons of our data and interpretations, including our quite relevant experiments on the production of 'masochistic' cats that avidly *seek* electroshocks. Instead, it may be of greater interest to comment on a general tendency among some psychologists (and, alas, not a few psychiatrists) to pre-empt a few words in the English language, take out a special patent as to their meaning and then attempt to live off the royalties. An example is Dr Wolpe's use of 'motivational conflict', an expression which he apparently wishes to restrict to a clash between 'learned feeding responses' and grid shocks. Actually, rational reflection must expand such terms to encompass any disruptive impasse between (*a*) any *dynamic configuration of accustomed adaptive patterns* (including, in a cat's universe, the opportunity to explore an experimental cage without being rudely electroshocked), and (*b*) any new set of experiences that runs counter to such expectations—and is thereby rendered operationally 'noxious' or 'traumatic'. Were this not true, human

[1] Publications with references to related work by the author and his associates are:

Neurosis and alcohol. *Amer. J. Psychiat.* ci, 389. (1944.)

Dynamic psychology of war-time communications and morale. *Dis. Nerv. System,* v, 101. (1944.)

Principles of Dynamic Psychiatry; including an Integrative Approach to Abnormal and Clinical Psychology; with a Glossary of Psychiatric Terms. Philadelphia: W. B. Saunders. (1946.)

Effects of cerebral electroshock on experimental neuroses in cats. *Amer. J. Psychiat.* civ, 92–9. (With Mary Grier Jacques.) (1947.)

Psychological medicine and world affairs. In Harris, Noel Gordon, ed.: *Modern Trends in Psychological Medicine.* New York: Hoeber. (1948.)

A biodynamic psychoanalytic approach to the problems of feeling and emotion. In Reymert, Martin L., ed.: *Feelings and Emotions. Second Mooseheart Symposium,* ch. iv, pp. 40–75. (1950.)

Effects of direct interrupted electroshock on experimental neuroses. *J. Nerv. Ment. Dis.* cxii, 384–92. (With Alex Arieff, Curtis Pechtel and Harold Klehr.) (1950.)

Experimental approaches to psychodynamic problems. *J. Mt Sinai Hosp.* xix, 639–52. (1953.)

Psychoanalysis and biodynamics—an integration. *Int. J. Psycho-Anal.* (Suppl.), xxxiv, 13–43. (1953.)

Role of olfaction in normal and neurotic behavior in animals. *Psychosom. Med.* xv, 396–404. (1953.)

Faith and delusion in psychotherapy. *Amer. J. Psychiat.* cx, 324–33. (1953).

Conflict-engendered neurotic and psychotic behavior in monkeys. *J. Nerv. Ment. Dis.* (In the Press.)

Neuroses in monkeys: a preliminary report on experimental observations. *Annals N.Y. Acad. Sci.* lvi (no. 2), 253–65. (With C. Pechtel.) (1953.)

neuroses would quite literally be confined to people who had either been caught in an electrical short-circuit or struck by lightning at the moment they were pushing an accustomed button to secure food from an automat.

As to Dr Wolpe's therapeutic inferences from his work: I regret that his recent publications and manuscripts are not as yet available here, so that I must be content to await further information from him with keen interest. Parenthetically, I do not quite understand what he means by 'treatment procedures...very different from those generated by Masserman's theory', since, in my *Principles of Dynamic Psychiatry*, based in part on our research, I tried to encompass all principal therapeutic methods from Ra to Rank. However, if Dr Wolpe will accept an exchange of stakes very likely in my favour, I shall wager a set of autographed copies of the publications listed above against correspondingly inscribed reprints of his forthcoming papers (*a*) that the therapeutic principles he derives from his studies are already widely known and applied, and (*b*) that they were probably also in use in the Asklepiad Sanatoria of the ancient Hellenes. And, as an instance of my trust in Dr Wolpe's scientific probity: if he will but consult the appropriate historical, experimental and clinical sources, he can be the sole judge of the matter.

I shall be glad to discuss the subject further with Dr Wolpe through more usual channels, but appreciate this opportunity to communicate and clarify our views.

Yours faithfully,

Jules H. Masserman

The Medical School
North Western University
Chicago
Illinois

CRITICAL NOTICE

By SIR GODFREY THOMSON

Uppsala Symposium on Psychological Factor Analysis, 17–19 March 1953. Nordisk Psychologi's Monograph 3. Distributed by Almquist and Wicksell Stockholm.

This three-day meeting of psychologists and statisticians was planned in order to further the understanding of their respective approaches to factor analysis. The remit was to discuss 'what psychology needs and what statistics can provide'. There were forty-four participants, including thirty-two from Sweden, five from Denmark, one from Norway and one from Finland. Of the other five, one each were from Princeton and Bombay, and three from the United Kingdom. These last three (Peel, M. S. Bartlett and Lawley) played a major part in the proceedings as the writers of invited addresses to the first three sessions. The other main papers were by Wold and Whittle of Uppsala and Rasch of Copenhagen. A paper by Lundberg of Lund was also briefly dealt with but is not available until it appears in *Psychometrika*. All the other papers are here printed except Whittle's which had already appeared in the Skandinavisk Aktuarietidskrift for 1952. The discussions are also reported.

It was a very important move in the development of factor analysis to bring some British statisticians who have shown interest in factor analysis into fertilizing contact with the famous Swedish School. Others will single out other names, but I was particularly impressed by reading that Cramér of Stockholm was present and took the chair at one session.

Peel's paper was a readable and illuminating review of past and present, not in the main mathematical. He suggested recommendations for the future, and the Symposium report ends with a valuable list of ten such, decided upon after general discussion.

Bartlett's paper was a very clear account of the mathematical history of factor analysis, and in his last three pages he adds another original contribution to the many he has made, namely a discussion of an attempt to transcend the hypothesis of linearity in the fundamental equation of factor analysis.

Lawley (the pioneer in the application of the method of maximum likelihood to factor analysis) gave an exposition of his new Method III, and Whittle's paper was another approach by maximum likelihood. Wold gave arithmetical illustrations of Whittle's method used on artificial test-scores.

The quite important papers of Rasch on simultaneous factor analysis in different populations, and of Lund on a ball and wire model, nevertheless stood somewhat apart from the main issue, which was on the papers by Lawley and Whittle.

Whittle's paper presents I think the most novel idea. He succeeded in estimating in one stage both the loadings of the factors in the tests, and the factor values possessed by the people tested. He does it by fitting, not the matrix of covariances or correlations, but the matrix of scores, an oblong matrix. He makes no assumption about the distribution of factor values in the population, but only about the ratios of the residual variances in each test. Lawley assumes, in addition, the factor values to be normally distributed. Their results are very similar but differ by a factor, which each confessed he was as yet unable to explain.

The ideas underlying Whittle's attack, as he says in his opening paragraphs (and indeed Lawley's too), go back to an article by Gale Young in *Psychometrika*, February 1941, and, I would add, to an earlier paper in that journal for September 1936 by Eckart and Gale Young, who discussed the problem of fitting most closely (least squares) one matrix by another of lower rank. In the case of a correlation matrix this is done by expressing the full matrix as a triple matrix product, with a diagonal matrix of its latent roots in the middle, flanked on both sides by the matrix of latent vectors. The best fitting matrix of a given lower rank is then obtained by replacing the necessary number of latent roots, beginning with the smallest, by zeros, and then multiplying the three matrices together.

This process gives new values not only to the correlations but also to the diagonal cells, and it interests me very much to observe that these values in the diagonal cells are larger than the communalities which would give the same lower rank. The 'specifics', that is, are smaller, and I have often protested against their very large variances produced by the method of communalities. The sum of squares of the discrepancies of the Eckart and Young approximation, *taken over all the cells including the diagonal*, is much less than that of the communality matrix, if there also the diagonal discrepancies are included.

So far I have spoken of a square matrix, but (and here is the link with Whittle) Eckart and Young also show how to apply these principles to an oblong matrix (such as a matrix of scores). Such an oblong matrix too can be expressed as a triple product, with a diagonal matrix in the middle (with however some extra columns of zeros). The entries in this diagonal matrix are the square roots of the latent roots of the covariance matrix made from the oblong matrix; and the pre-multiplying matrix contains the orthogonalized latent vectors of that covariance matrix. The post-multiplying matrix is found by solving for it in the matrix equation, and turns out to be a horizontal slab of an orthogonal matrix. If the dimensions of the oblong matrix were n tests and N persons, the dimensions of the three matrices are

$$(n \times n)\,(n \times N)\,(N \times N) = (n \times N),$$

but the last $N-n$ columns of the middle matrix are all zeros, and the last $N-n$ rows of the post-multiplying matrix are not required, and need not be found. The best-fitting matrix of lower rank r can then be found by replacing all except the r largest entries in the middle matrix by zeros and multiplying the three matrices together.

Five years later Young (after referring to Lawley's Method I) applied this plan, using maximum likelihood, to test-score matrices, assuming that the errors are normally distributed but that the matrix of the reciprocals of the standard deviations of the errors is of rank one. He knew of no solution to the more general case. It appears to be here that Whittle's paper fits in, and Lawley's Methods II and III. Whittle describes Method II as 'an appropriate extension of Young's procedure'; but it broke down, and Method III takes its place.

Young speaks of 'errors' where Lawley speaks of 'specifics' and Whittle of 'residuals', all meaning the same defect from full variance in the diagonal cells. Lawley at the outset of his Uppsala paper speaks of these differences, for they are more than merely verbal. There are, he said, two models in use. In the first (Whittle's) both the loadings and the factors are treated as parameters to be estimated rather than as variates. The residuals are assumed to be independent random errors normally distributed.

In the second model (Lawley's) these residuals represent not only error but also specific factors. If the same assumption of normal distribution is made for these specific factors it seems reasonable to make it about the common factors, which are therefore to be treated as independent normal variates, leaving only the loadings as parameters to be estimated. Wold, in discussion, called Lawley's analysis a special case of Whittle's, but Lawley did not agree. 'There is surely', he said, 'a deeprooted distinction between the conceptions of the factor values as statistical variates and as systematic parameters.'

About the fact that these two methods differ by a factor, Bartlett said that this may be analogous to the difference between his own method of factor estimation and Thomson's (i.e. mine). Bartlett's minimize over the specifics, mine ensure that the mean square deviations of the factor estimates are a minimum over all persons in a group having the same set of scores. In both applications the size of the specific factors has already been decided in order to reduce the rank of the matrix. It would be interesting to make some arithmetical comparisons, helped by Wold's paper.

These methods of Lawley and of Whittle resemble closely a principal components analysis. When the residual or specific variances are all equal they reduce exactly to a principal component analysis. It is likely that in orthodox factor analysis Lawley's Method III will be preferred to Whittle's method, because it reproduces the correlations better. All depends of course on whether we should give pride of place to this, or to reproducing the whole matrix, including the diagonal, by one of lower rank.

Lawley's Method I is perhaps still more likely to be preferred by factor analysts. It works perfectly well in practice except only in cases where 'impossible' matrices crop up, which, as Bartlett said at Uppsala, does not invalidate the method. Lawley has not yet been able to give a fully rigorous proof that it leads to 'efficient' statistics, but it seems almost certain that it does so.

The Uppsala Symposium is a milestone in the progress of factor analysis. What I would like to happen next is an elementary seminar with explanations as simple as possible of the points discussed at such a high level at Uppsala. The members would have to be given prerequisite reading, and examined on it!

PUBLICATIONS RECENTLY RECEIVED

Selection for Industrial Leadership. By A. G. Arbous. London: Oxford University Press. 1954. Pp. xvi+180. 30*s.*

In 1948 the South African National Institute for Personnel Research was called on to assist in selecting a small number of administrative trainees, that is potential high-grade executives, for a large industrial corporation. A procedure very similar to that of our own Civil Service Selection Boards (though somewhat less thorough, for reasons outside the Institute's control) was applied to 219 candidates, of whom 16 were selected. Since, however, more than two-thirds of the candidates were already in the corporation's employment, it was possible to follow up a population of 116 rejected as well as the 16 selected candidates, and to obtain comparable ratings a year later on their potentialities as administrators, thus by-passing the very difficult problem of correcting follow-up results for selectivity. An adaptation of the Otis Group Test yielded a validity of 0·53, Matrices 0·43, assessments of a written project (similar to the CSSB 'Island Story') 0·53, and assessments of group discussion and committee sessions 0·60. The multiple correlation based on these reached 0·68. However, this probably represents somewhat poorer validity than that obtained in CSSB selection, since the variance of the candidates in *g* and education was obviously unusually large. Though several other interesting techniques were employed—a general knowledge test, an employment interview by each candidate with a 'stooge', a clinical interview by a psychologist, and a sociometric questionnaire—their possible contributions to the procedure are not indicated. A subsidiary study in another organization suggested that the corresponding validity of conventional selection by application blank, testimonials and Board interview, was only of the order of 0·20.

The value of the book lies less in the psychological results that we have outlined than in its review of the literature of group methods, and its contributions to the statistical methodology of selection. The former provides an excellent and critical survey of WOSB, CSSB and OSS work, though exception must be taken to one quite crucial point—namely the applicability of the formula for indirect selectivity. Arbous argues that in CSSB selection, for example, the follow-up group was selected on a subjective and unspecifiable combination of variables. True, the Final Selection Board mark was derived from such a complex combination, but the selection—so far as the statistician is concerned—was based exclusively on the FSB mark, and therefore satisfies the conditions for applying this supremely useful technique.

Nearly half the volume deals with the interpretation of validity coefficients which, as Arbous makes clear, have little meaning apart from the practical conditions of application of the selection procedure. Everything depends on the proportion of candidates it is proposed to select, and on the training wastage and wastage of wrongly rejected candidates that can be permitted. Techniques, and specimen graphs, are given so that employers can determine the precise effects of putting the procedure into practice (but though the description is lucid, an employer would need a good deal of statistical training to follow it). The reviewer is not entirely convinced of the value of such techniques, since validity coefficients (particularly multiple ones) vary so much from one sample to another. Moreover, the very common situation where the selector expects an improvement in the quality of selectees by raising the pass-mark (e.g. all educational selection) is not covered. Nevertheless, Arbous's approach is undoubtely stimulating, and any psychologist concerned with selection problems would profit from studying his contributions.

P.E.V.

The Geometry of Mental Measurement. By Sir Godfrey H. Thomson. London: The University of London Press Ltd. 1954. Pp. 60. 6*s.* 6*d.*

This little book is based on three lectures given by Sir Godfrey Thomson in London University in January 1953. His aim is to construct a geometrical model in terms of which the main formulae used in the statistical analysis of test results can be interpreted. Any psychologist who has a slight mathematical background and is 'willing to think spatially' should certainly read it, for the author's lucid style and omission of any irrelevant details will help many readers to appreciate in a new way the significance of techniques with which they are already familiar.

The correlation between two tests is envisaged as the cosine of the angle between two axes (representing the tests); starting from this basic idea the author leads on to a discussion of partial correlation, regression

and factor analysis, in terms of angles and projection in two or more dimensions. By confining himself to the extraction of one factor, with only three tests, he shows clearly the geometrical meanings of 'communalities' and 'factor loadings', and the problems involved in the estimation of factors. The author does not concern himself here with sampling problems, calculations, rotation of axes, or the psychological interpretation of the relationships discussed; his treatment is essentially abstract and, for the geometrically inclined, it is very refreshing.

M. A. CREASY

Rituals of Rebellion in South-east Africa. By MAX GLUCKMAN. Manchester: Manchester University Press. 1954. Pp. 36. 3*s.* 6*d.*

Social tensions, it is now fashionable to hold, are bad; much writing and investigation is directed towards the discovery of reducing or eliminating tensions in society. This assumption that human society would work better without tension, conflict and frustration is rejected by most social anthropologists; in their analysis of social systems they assert that cohesion in a society is achieved by the mutual opposition of parts and by the pressure of external enemies; 'a society is knit together by its inner cleavages'. Among most primitive peoples men are agreed about the right form of society and struggle for power within it; ambition leads them to become rebels, not revolutionaries, and usurpers, not heretics, so that every rebellion is a reaffirmation of the rightness of the existing order. The latent and enduring rivalries and conflicts also find expression at annual ceremonies, when subjects insult and taunt their rulers, princes challenge the king, and women pantomime the roles of men.

In this Frazer Lecture for 1952 Prof. Gluckman examines certain agricultural ceremonies in which social conflicts are vividly dramatized, and contrasts them with those of our own society in which 'royalty is blatantly extolled, the virtues of the nation are magnified and the country is glorified'. He argues that 'every social system is a field of tension, full of ambivalence, of co-operation and contrasting struggle', and that where the common moral order is not questioned, men emphasize by acting out the conflicts either directly or symbolically, the social cohesion within which the conflicts exist. Where the social order is itself questioned, as in Western society, political ceremonies cannot be used for catharsis in this way.

This short work is scattered with suggestions about the connexion between hostility and social cohesion, and should be read by all who maintain that tension and conflict are always socially destructive.

J. A. BARNES

The African Mind in Health and Disease. A Study in Ethnopsychiatry. By J. C. CAROTHERS. World Health Organization, Geneva. 1953. Pp. 177. 10*s.*

It is a pity that an excellent review of the psychology and psychiatry of the African should have been spoiled by premature generalization. The literature of the subject is well covered—though the anthropologists might have been given more space in view of the importance of the argument from culture that occupies much of the book. There is certainly no other work which so well expounds what we know of the African. But how little we do know is clear from the short bibliography of only 191 items, covering not only the psychology and psychiatry of the African, but also his nutrition, culture, physical anthropology and ethnology. The paucity of material should, perhaps, have warned the author of the danger of generalization.

His generalization is that the African in Africa south of the Sahara is more like his fellows in that enormous continent than a European is like other Europeans. We are told that he is similar both in culture and personality, and that the latter is mainly the product of the former. It may be that both these generalizations are true, but if they are then they must be counted as very lucky hunches—or penetrating insights—for the evidence the author advances is not at all convincing.

For example, he says that children are much desired by African parents, are indulged up to the time of weaning and that from birth they live in a smooth, warm rhythmic world not far removed from the prenatal one. No authorities are given for this statement, which is meant to apply to the whole of southern Africa and to all individuals. But a few sentences below this (p. 43) it is asserted that there is another side to the picture; that young children are often left in charge of a small sister quite unfitted for the responsibility, and that they may be forced against their will to eat unsuitable foods. The author implies that the indulgence is of major importance whilst the frustrations which may be produced by the young sister are unimportant. But he gives no evidence of either the amount or relative importance of

the two kinds of treatment, and neither does he tell us in what precise ways the sister may frustrate the young child.

All discussion of the cultural environment is of this kind. Carothers merely asserts the cultural conditions to be of a certain kind, and, what is more dangerous, asserts that they have specific psychological effects. Weaning, for example, he says will be very traumatic to the African child. This assertion is taken from the study of Ritchie, which itself is based largely on impressionistic data and was conducted in a very small area of Africa. Your reviewer has just concluded a detailed study of sixteen African children and has found that the dire conditions asserted by Ritchie to result from weaning do not appear. By the seventh week after weaning the child appears little affected by the process and, indeed, there is some evidence that his development towards maturity has been aided. Also, it is clear that at eighteen months the African child is almost ready to wean himself.

The generalizations about the psychology of the African are based on the work of seventeen authors. Much of this work is very restricted both in numbers and scope, not all of it is empirical and some is frankly anecdotal. To generalize about the African from such data is dangerous, and becomes more evidently so when it is realized that most work has been done on intelligence, and in this exact field the results are doubtful. If it is difficult to establish differences by means of well-developed techniques, how much more difficult it will be to be certain of results obtained with less exact methods.

The generalization that the African suffers from a 'fragility of the higher psychic functions...' may be a good guess but it is unwarranted to attribute this disturbance to the frontal lobes when we know so little about frontal lobe function.

This book is perhaps best regarded as providing us with an excellent review of the psychology of the African together with yet another stereotype. The stereotype presented does not, after all, differ markedly from the ordinary one of the African as a shiftless, lazy and happy-go-lucky soul. It may be that he is such a person, but a great deal more evidence than we have is necessary to establish this as a fact.

R. C. ALBINO

The Integration of Behaviour, Vols. I and II. By THOMAS M. FRENCH. Chicago: University of Chicago Press; London: Cambridge University Press. 1954. Pp. xii+272. 37*s.* 6*d.* net (vol. I). Pp. xii+367. 49*s.* net (vol. II).

Anyone undertaking to write a series of five volumes intended to elucidate the integration of behaviour must at least be allowed a sense of adventure. Dr French is well endowed with this sense. This is clear from the two volumes of the series which have so far appeared.

His basic assumption is this: the student of behaviour must first analyse it into its supposed elementary reaction patterns before attempting to demonstrate how these are combined. Dr French endeavours to identify these elementary patterns by a detailed study of a patient (not his own) during two years of analysis. His declared aim is not to prove anything but the more modest one of illustrating a method.

The first volume has been given the subtitle 'Basic Postulates'. The choice of this subtitle remains a mystery for it covers a wide miscellany of topics, including studies of frustration, the learning process in therapy, and the influence of sleep. The second volume explores the integrative process in dreams. Here we must pay tribute to Dr French for his courage in coming to grips with a problem of outstanding importance, though the academic psychologist does not recognize it as such, in spite of the fact that he dreams away at least a third of his life. Dr French here assumes a twofold task: first to relate the different possible meanings of a dream to one another and to show how they fit together in a closely-knit logical structure, and second to reveal how different dreams of the same person are connected. He undoubtedly succeeds in making many illuminating comparisons between the patient's dreams dreamed on different occasions and in varying circumstances.

Dr French draws inspiration from many sources, chiefly from Freud but also from Lewin, Tolman and even Hull. He employs a Hullian type of symbolism the esoteric character of which is likely to restrict its appeal, and his conception of 'integrative capacity', i.e. the amount of influence on behaviour of a particular insight at a particular time, is conceived on analogy with Hull's notion of habit strength.

Dr French would have employed his clinical gifts to better purpose if he had confined them to a more limited goal. He seems to be out of his theoretical depth. The reader would be willing to sacrifice a little integration of behaviour in exchange for some integration of theory. The fifty chapters in the first volume and the fifty-nine chapters in the second are often notes and unintegrated fragments rather than chapters. The thinking which should have been done by the author is left to the reader.

In the three volumes yet to come Dr French proposes to discuss variations in the patient's behaviour from time to time. To prejudge these future volumes would be unjust. For the author's sake it is to be hoped that they will be more coherent than their predecessors.

JOHN COHEN

Introduction to General Psychology. By E. J. Asher, Joseph Tiffin and F. B. Knight. Boston: D. C. Heath and Co.; London: G. G. Harrap and Co. Ltd. 1953. Pp. xvi + 515. 25*s*.

The appearance of a new text-book on General Psychology is always an interesting event. In this volume the authors have presented a balanced survey of contemporary psychology, arranging the various subtopics (chapters) in a logical order. Emphasis is placed upon psychology as an empirical science. While ample factual material is presented, even in this introductory treatment the authors very rightly find space to raise some of the controversial issues in contemporary psychology. A certain amount of over-simplification of complex issues is inevitable. Whilst the reviewer would not himself rely on any single existing text for the beginner, he would add the present volume to the limited list of those which might well be adopted for such introductory courses. Amongst those responsible in the U.K. for initiating the beginner into the mysteries of modern psychology some may consider the book too long, but it is difficult to see how the same amount of material could be condensed further. Like many texts in use, this naturally reflects the American 'culture pattern'. The chapter on 'Abilities and Aptitudes' places considerable emphasis on apparatus tests which will seem to some of us to belong to the 1930's. However, the chapter is rounded off with a useful and critical discussion of the interpretation of test results. The diagrams and other illustrations in the book are simple, clear and informative, and the general layout is excellent. By modern standards the price of twenty-five shillings certainly represents good value for money, a point which will recommend the book to the student, as one which he might purchase.

ROLAND HARPER

Human Behaviour: Psychology as a Bio-Social Science. By Lawrence E. Cole. New York: World Book Co.; London: G. G. Harrap and Co. Ltd. 1953. Pp. xii + 884 37*s*. 6*d*. net.

Developmental Psychology. By Elizabeth B. Hurlock. New York, Toronto and London: McGraw-Hill Book Co. Inc. 1953. Pp. x + 556. 48*s*.

Essentially, *Human Behaviour* is an expanded and more up-to-date version of the author's deservedly popular *General Psychology*. Despite its title, it contains a good deal of information about animal experiments, and though it is thin on physiological psychology it is otherwise notably comprehensive. The book has no particular theoretical standpoint, the 'bio-social' approach being confined mainly to the rather laboured paragraphs which open some of the chapters. There is a certain lack of perspective—inevitable, perhaps, when so wide a field is covered by a single writer—but this is a minor defect in a book which most readers will treat mainly as a work of reference.

One of the most valuable features of *General Psychology* is the clear and balanced account of Freudian theory which is concealed in the chapter on 'Thinking'. This account has been expanded in the final section of *Human Behaviour*, but part of the new material is noticeably inferior in quality: the content is slight, and the clear and workmanlike style of the rest of the book degenerates into 'colourful' journalese in which, for example, Janet's exhaustion concept is said to be, in Freud's opinion, 'as far from the harsh realities that go on within the mental apparatus as a prettified chromo from the dear, dead past'. However, these unfortunate pages, with their dragged-in references to Pareto and Niebuhr and Kierkegaard, form only a small part of a long, meaty, lucid and thoroughly competent textbook.

Developmental Psychology has little truck with depth-psychology. It is a full and systematic account of the changes, physical and mental, involved in growth and senescence, and it contains some excellent advice, at the commonsense level, on how best to maintain adjustment at the various stages ('adjustment' is the author's favourite word). Dr Hurlock is not afraid of the obvious, e.g. 'Divorce is an indication of poor marital adjustment.... Legal separations and desertion are likewise indications of poor marital adjustment'; but she is conspicuously lucid and sensible, and her book, though described by the publishers as a second-year text-book, should be a valuable source of information and advice to the layman.

MARGARET KNIGHT

Psychoanalytic Theories of Personality. By GERALD S. BLUM. New York, Toronto and London: McGraw-Hill Book Co. Inc. 1953. Pp. xviii+219. $3.75, 32*s*.

This is an extremely learned summary of the theories of Freud, and of practically all those of his followers as have diverged from him in any way. It is arranged in the order of the individual's development from pre-natal life to adulthood, and is liberally illustrated with the results of experimental work, beginning with the increase in foetal heart-beat and movement in response to sharp, loud noises, and going up to such studies as an attempt to relate oral traits in adults, as measured by a self-rating scale, to age of weaning. It is not illustrated at all with the case-material which is the principal source of the psychoanalyst's knowledge. A beginner attempting to use it as a short-cut to such knowledge, or even as a guide to further reading, would gain little understanding, and would be bewildered by a book in which the profound discoveries of Jung and Melanie Klein are treated on a level with scores of minor variations of Freudian theory propounded by relatively unimportant people. For anyone, however, who has a fair amount of psychoanalytic insight, it will be valuable as a refresher course, a reference book, and a stimulus to thought and further discovery.

JANE DARROCH

Hypnotism: an Objective Study in Suggestibility. By A. M. WEITZENHOFFER. New York: John Wiley and Sons, Inc.; London: Chapman and Hall Ltd. 1953. Pp. xvi+380. 48*s*.

This book is the first real attempt to present all the available experimental evidence in this field since C. L. Hull published his classic *Hypnosis and Suggestibility: An Experimental Approach.* In view of the mass of material that has accumulated since, the attempt was certainly well worth making, and it must be judged to have been, on the whole, remarkably successful. The author, aided by his training in the exact sciences, has a good eye for well-controlled experimentation, a critical mind, is well read in the subject, and obviously has personal experience in the field.

The documentation is very thorough, the bibliography running to over 500 titles. Of major omissions which could alter the author's conclusions I have only been able to find one. In his discussion of hypnosis and dissociation on page 99, Weitzenhoffer quotes the work of Mitchell, which is negative, but not that of Nagge, which is positive, and being derived from a larger number of cases appears more trustworthy. This is an important point as Weitzenhoffer bases some of his theoretical conclusions on this study.

Welcome as the detailed discussion of the experimental literature is, the most important contribution Weitzenhoffer has to offer is probably his theory of the hypnotic process. He eschews monistic theories and puts forward the view that 'Hypnosis is the outcome of not one but of several temporally overlapping and interacting processes'. The two main ones the author considers to be those of *homoaction* and *generalization*; in the later stages of hypnosis a third process, namely a narrowing of consciousness, is introduced. The theory is ingenious and has the usual advantages and disadvantages of theories involving the interaction of several processes, none of which can be specified or measured with any degree of accuracy. It would probably be possible to account for most hypnotic phenomena along these lines, but it would be very difficult to make predictions which could invalidate this theory. In spite of this criticism, theoretical formulations in the field of hypnotic phenomena have generally been so obviously untenable that Weitzenhoffer's formulation stands out as a much more reasonable interpretation of the very complex phenomena under discussion than any alternative theory known to the reviewer.

H. J. EYSENCK

Trauma, Growth and Personality. By PHYLLIS GREENACRE. The International Psycho-Analytical Library and the Institute of Psycho-Analysis. The Hogarth Press 1953. Pp. xi+308. 30*s*. Ed. by Ernest Jones.

This volume is a selection of papers made during the past decade and published in various journals. They have a special interest because the author, Dr Greenacre, is a clinician through and through, which means that what she writes, even when it could be better written and even when it is without form, is never without interest. Fortunately Dr Greenacre has wide clinical experience which includes paediatrics and especially the care of infants and of newborn infants. She also has experience of adult psychiatry from the mental hospital angle and to this she has added the psychoanalytic training and many years of full-blooded psychoanalytic work. Moreover, in the latter she has undertaken treatments of borderline

cases where the matter of management cannot be thought of as outside the analytic work since the patient of whatever age is at times an infant in the transference situation. If the patient is being a newborn infant or even an infant acting out birth memories Dr Greenacre knows where she is because of her exceptionally wide experience apart from that gained in doing classical psychoanalysis.

The first chapter on The Biological Economy of Birth (1945) interests me most, because it is unlikely that there are more than a very few properly trained and fully experienced analysts who are competent to refer to the phenomena of actual birth. One starts with the stimulus provided by Freud's suggestion that the phenomena of anxiety are related to the phenomena of the birth experience. Freud however was not (at any rate in the early days of psychoanalysis) thinking of the individual's memories but of a build-up of the birth experiences of a million years in the race of human beings. The difficulty is to know how much of the human psyche is present at the time of birth. One can describe the phenomena of the birth trauma but many of these are present even in the case of an anencephalic monster. Appearances therefore are deceptive. At the other extreme from anencephaly there are the adult's fantasies about birth which are notoriously coloured by knowledge gained at later dates, so that whenever birth experiences are remembered they are liable to be mixed up with pseudo-memories and even with clear visual impressions which cannot possibly have been based on the sight memories of the birth era. Complex fantasies of birth, however, do not exclude the possibility of birth memories, and what is needed if progress is to be made is knowledge of the psyche of the human infant at the time of birth so that the meaning of the traumata to the infant can be assessed. I think there is no study of this subject at all comparable to that which appears in the first chapter of this book and also in the next on The Predisposition to Anxiety. No attempt is made to reach a final conclusion but the subject is opened up and the author is capable of making sane and objective judgements through being an observer of infants at the same time as being a psycho-analyst with a grip on psychoanalytic theory. Room is left for the conception of a normal birth and for a specific anxiety pattern that may be associated with birth by Caesarian section.

It would seem to me that Dr Greenacre could enrich her work by bringing to her study of the problem the conception of environmental adaptation as something which at the beginning belongs to the infant. If the infant is at first not differentiated from the environment, then environmental adaptation, if good enough, is part of the infant, although as we see it this adaptation is the work of a mother who at the beginning loves in physical terms. In describing the management of patients who seem to be dealing with birth trauma in the transference situation Dr Greenacre describes her own technique but she does not actually join this up in theory with the primary narcissism of the infant, explaining that in her management she is acting a part of the individual infant before individuation has become a fact.

In further chapters Dr Greenacre's wide experience continues to enrich her studies of the early stages of the development of female sexuality with a special emphasis on the effect of anatomical structure on the super-ego formation which is in some respects different in girls from what it is in boys. Also there is a valuable comment on the effect of assault on girls in the pre-puberty age.

For the clinician this is an exceptionally valuable addition to the International Psycho-Analytical Library. Psychoanalytic theory is basically accepted and yet is opened up for modification and development. This comes from the author's clinical experience in which she brings together in her technique for the analysis of borderline cases a true appreciation of classical psychoanalysis and first-hand experience in the management problems of early infancy and of birth. D. W. WINNICOTT

Psychological Reflections. By C. G. JUNG. Collected and Edited by Jolande Jacobi. London: Routledge and Kegan Paul. 1953. Pp. xxviii+342. 30*s.* net.

This anthology, collected and edited by Dr Jolande Jacobi, was first published in Zurich in 1945, the year of Jung's seventieth birthday. Dr Jacobi set herself the considerable task of presenting to the general reader a panoramic view of Jung's contribution to modern thought.

The book contains a selection of over a thousand quotations. They are mostly statements of a general nature that can stand by themselves. Within a framework consisting of sections and subsections, the quotations are arranged, not in chronological order, but in meaningful sequences designed to illustrate particular themes in the most appropriate way.

In this book Jung the philosopher takes precedence over the clinician, and the subject-matter of the quotations deals with wide spheres of human life and activity. The nature of the psyche, primordial images, dreams, men and women, problems of self-realization, the life of the Spirit, Fate, Death and Renewal—all these and many others.

This is not a book that can be read easily or quickly, but it has been most thoughtfully compiled. For

those who can digest general conclusions without demanding the particular evidence for them, this book succeeds in conveying the nature and quality of Jung's contribution to modern thought more vividly than any summary of his work could possibly do.

Many things have been said, and will continue to be said, for and against Jung. He is, *par excellence*, a controversial figure; inspiring to some and infuriating to others. But, as Dr Jacobi remarks in her preface, 'No-one who does not stand aside from real life will be able to avoid coming to terms with Jung, whether the attitude be one of agreement or disagreement.'

Theoretical Models and Personality Theory. Edited by D. Krech and G. S. Klein. Durham (N. Carolina): Duke University Press; London: Cambridge University Press. 1952. Pp. 142. 19*s*.

Reviewing a symposium is often a puzzling task. One of its difficulties is that the individual papers must be assessed not only as separate items, but also as contributions to a whole—and a whole, moreover, whose aim may not be well-defined. In this volume a general discussion of theoretical models by von Bertalanffy is followed by papers in which Hebb, Rapaport, Miller, Eysenck, Halstead and Angyal describe and defend the type of psychological model which they prefer—the neurological, psychoanalytic, Hullian, factorial, organismic. What they have to say is in general well said, but in its broader aspects hardly novel; the positions are familiar, and one is led to wonder why it was thought worth while to bring them together under this title.

Some part of the answer is perhaps to be found in the opening paper by the editors, who deny that there is any distinct body of theory which can be called 'personality theory' and set slongside other subsystems such as 'learning theory' or 'perception'. They very correctly point out that the existence of these subsystems is itself a barrier to theoretical advance in psychology; and that personologists who, by adopting such slogans as 'personality and perception', perpetuate these divisions, delay the construction of the unifying framework of behaviour theory within which they should properly seek the answer to their own questions. Klein and Krech then attempt to state the requirements of such a behaviour theory.

The correctness of their requirements, and the extent to which any of the proposed models can meet them, are questions which might profitably have been taken up by the later contributors; as it is, they have simply been passed to the reader. Can the title of a symposium not be made to function as something more than a projective device? N.E.L.

Affective Disorders. Psychoanalytic Contributions to their Study. Edited by Phyllis Greenacre. New York International Universities Press Inc. 1953. Pp. 212. $3.00.

As Dr Greenacre tells us in a short Foreword, this book, which contains contributions by five other psychoanalysts, represents part of certain 'panel discussions' held at meetings of the American Psychoanalytic Association in 1950 and 1951. She regards it as 'a heartening indication of the growth of analytic vision that it again concerns itself with psychoses and borderline states, although these conditions are not generally amenable to formal psychoanalytic technique, but require special flexibilities, modifications and empathy'. The 'concern' may indeed be praiseworthy, but it seems doubtful whether the results so far obtained, to judge by the contents of this book, will be generally considered 'heartening'. The present discussions do not touch, at least directly, on therapy, but are concerned with theoretical formulations—in terms of 'metapsychology'—regarding the processes operative in depressive or manic states. The reader is left with the impression that little definite progress along these lines has really been achieved in recent years. The psychoanalytically orientated psychiatrist will probably be interested to see how far the very tentative formulations put forward by the discussants can throw light upon his own experience, but for most others the book may well appear both difficult and depressing. To them the writers may seem to be only too often engaged in rather unsuccessful manipulations of the concepts 'ego', 'id', 'super-ego', 'orality', 'pleasure principle', 'projection', 'introjection', etc., in the hope that they can be placed into such a relation to each other that they will explain what happens when a patient succumbs to melancholia, or mania, when he changes from one state to the other, or when he recovers. They appear to be grappling, as it were, with a very refractory jigsaw puzzle.

The difficulty is increased by the fact that the use of these terms is not always quite consistent or in accordance with tradition; as though indeed the pieces of the jigsaw were liable to change their shapes

when handled. To give just two examples: On pp. 25–26 Dr Edward Bibring tells us that in his view depression is 'not determined by a conflict between the ego and the id or the super-ego... but stems primarily from a tension within the ego itself... since it feels unable to live up to its aspirations (ego ideal, superego) while they are strongly maintained'; here the words within brackets seem in contradiction to what goes before. On p. 146 Dr M. Katan says that 'the superego, as the representative of reality, is the most important pillar of the reality principle', and adds no explanation or qualification, in spite of the generally recognized existence of powerful archaic and unrealistic aspects of the super-ego, and in spite of the fact that some thirty years ago Freud in *The Ego and the Id* (p. 34) expressly repudiated his earlier view that the super-ego included the 'testing of reality' among its functions.

It is clear both from Dr Bibring's introductory paper and from those that follow that the writers no longer consider that oral fixation, incorporation of a lost love object, turning of aggression against the self, and ego-super-ego conflict, play quite such an important role in the production of melancholia as they seemed to do before, though there appears to be little general agreement as to what further explanations are required. Dr Bibring regards the ego's sense of helplessness as the essential factor. Dr Edith Jacobson is perhaps nearer the 'classic' view when she stresses what she calls the 'symbiotic' type of relationship—involving dependence on supplies of (oral) love—as predisposing to depression, though she also thinks that at the onset of psychosis proper there is a defusion of the libidinous and destructive forces, leading to a 'fatal libidinous impoverishment and an accumulation of sheer aggression'. She would seem to be thinking here in terms of Freud's Eros and Thanatos. Dr Elizabeth Rosenberg Zetzel, however, while accepting the importance of Melanie Klein's 'depressive position' in infancy, does not consider that this necessarily involves the operation of a 'death instinct'. Dr Gearge Gero, in a study of anorexia, is inclined to find a cycloid process in which the super-ego first curbs the longing for love, this being followed by an unavailing appeal by the 'depleted ego' for love from the super-ego itself; while Dr Katan, in considering mania, holds that the predominant working of the pleasure principle has a therapeutic value in the patient's effort towards recovery, by healing the 'narcissistic wound' and 'causing the surplus of id agression to disappear', though he is quite frankly vague about the dynamics of the processes involved.

The general trend of the discussion is perhaps best indicated by Dr Gero when he says that at the present time 'We are much less apt to reach easy generalizations but much more able to develop concepts and correlations which are descriptive of the unending varieties of the phenomena presented to us by our patients'. We must however, it would seem, resign ourselves to the fact that these concepts and correlations have as yet hardly advanced beyond the stage of trial and error groping. J.C.F.

Psychoanalytic Studies of the Personality. By W. Ronald D. Fairbairn. London: Tavistock Publications. 1952. Pp. xi+312. 25*s*.

The author presents a highly individual contribution to psychoanalytic theory under a misleadingly general title. He offers an object-relations theory of personality, ousting the Freudian libido from its fundamental place, and assuming, like Klein, a fundamental schizoid split in all personalities. Freud's libido as pleasure seeking is supplanted by Fairbairn's libido as object-seeking, and the concept of endopsychic structure which derives from this object-relations theory is conventionally tripartite. A central ego and two subsidiary egos (libidinal ego and internal saboteur) correspond superficially to ego, id and super-ego, but the ultimate difference lies in the fact that Fairbairn bases endopsychic structure upon repression of internalized objects. The important practical and theoretical inference is that the ego is inherent and that the subsidiary egos are derivatives from this original unitary ego, as opposed to the Freudian position of the development of the ego from the undifferentiated id. To Fairbairn, id, or libidinal ego, is a split-off portion of the original dynamic ego. Abraham's theory of libidinal development is replaced by a psychopathology of the psychoses and psychoneuroses using the processes of accepting or rejecting, internalizing or externalizing objects, and based on a scheme of development from infantile dependence through the transitional stage of quasi-dependence to mature dependence.

The theory stems from the author's sensitive clinical experience and insights. Thus his work, while mainly relevant to psychoanalysis, has something for all interested in the dynamics of personality. To the general psychologist, the formulation leading to the concept of a unitary, inherent or original ego is important, but the less specialized reader would be well-advised to provide himself with a background of neo-Freudian development before tackling this book.

Dr Fairbairn does himself an injustice in the presentation of his views in this book. He collects his articles and essays over the last twenty-five years from various sources and connects them by footnotes and addenda. This arrangement, however, permits too many minor contradictions, alternations and repetitions to distract the reader from the main theme of the book. R. M. MOWBRAY

Aggression and its Interpretation. By Lydia Jackson. London: Methuen. 1954. Pp. 237. 21*s*.

The title of this book promises a more comprehensive treatment of the subject than the author in fact gives us. The main part consists in an account of two experiments and some therapeutic work with children in the phases of latency and early adolescence. These studies are prefaced by a sketchy discussion of theories of aggression. The vexed question to which the author addresses herself is that of the origin and transformation of aggression, her thesis being that 'aggression is a manifestation of self-assertion...grounded in the survival urge'. One may doubt whether any empirical studies can either confirm or refute such a broad quasi-biological hypothesis and, indeed, the link between the author's theory and her experimental and clinical observations is not a close one.

In the main study reported, projective tests of family attitudes were administered to normal, neurotic and delinquent children, the normal subjects giving a much more positive picture of family relations than either of the disturbed groups. The finding that the delinquents admit less hostility than neurotics to parents and sibling-figures, idealizing the absent family, is difficult to interpret since more of the delinquents than neurotics seem to have been living in institutions at the time.

The clinical studies of behaviour under the influence of a cathartic type of play therapy provide many interesting examples of overt aggression giving way to more constructive forms of relation, but the therapist's role is so vaguely described that it is hard to understand the dynamics of these changes.

In style the book is rather conversational and would have profited by a good deal of 'cutting'.

CECILY DE MONCHAUX

Aggression, Hostility and Anxiety in Children. By Lauretta Bender. Springfield, Illinois: Charles C. Thomas. 1953. Pp. viii+184, 23 illustrations. 40*s*.

This expensive volume is beautifully produced and leads the reader to expect much. In my opinion the expectation is not fulfilled; nevertheless a great deal of good clinical work goes to the making of the book and anyone studying the problems brought forward will not be able to miss reading it. There are seven separate studies written by Paul Schilder and Lauretta Bender and their associates at the Bellevue Hospital. These deal respectively with Aggression, Death, Suicide, Homicide and Firesetting, as these appear in child psychiatry. The genesis of hostility is discussed. There is a final chapter on Anxiety.

In the studies on aggression and death, projection tests are used. In the latter case there is a series of pictures and in the reviewer's opinion one of these is quite unsuitable for a test to be used with children. It shows a number of men and women hanging. One is used to every kind of horror that appears in the children's own drawings of their dreams and conscious fantasies, but it is quite another matter to present children with reality horrors which many mature adults must find disturbing. It is a sign of the times, one supposes, that the official description of this picture is 'Russian spies hanging from gallows'. In this country both the picture and the description would be wholly out of place. I hope it will be understood that this criticism does not imply any scotomization of the capacity of the child both normal and abnormal to conceive of the horrible.

In each section rich and interesting surface descriptions are given and these can have value to those who are not able themselves to be in touch with clinical material. Anyone clinically involved would of course derive more from the careful study of a few personal cases than from the perusal of a large collection of cases described by other observers. What is needed from this book is new light on the origin of aggression. Readers will seek for this in the last two chapters where American authors are well quoted and also Melanie Klein. In the reviewer's opinion there is no possibility of a valuable new idea in this book because of what seems like determination on the part of the authors to avoid a developmental approach. Quotations from Freud and Melanie Klein particularly show the way in which statements are taken without a full appreciation of their meaning in relation to development.

A comment on Melanie Klein's theory on page 145 that anxiety springs directly from aggression which the authors find 'conclusively disproved' seems to show no knowledge of Klein's complex statement of the depressive position in normal emotional development. In place of the general psychoanalytic statement of the developing individual, one gets a general theory that aggression in childhood is a 'symptom complex resulting from deprivations which are caused by developmental discrepancies in the total personality structure, such that the constructive pattern drives for action in the child find inadequate means of satisfaction....' Here the word development occurs but the idea does not seem to be followed through.

The best part of the book is an unexpected section at the end on schizophrenia in childhood. Here however the operative factor seems to be the birth experience. If the authors become able to make a statement of *what the infant is like* when affected by birth experiences they will then be able to re-state their theories in a more convincing way. D. W. WINNICOTT

Child Training and Personality: by JOHN W. M. WHITING and IRVIN L. CHILD. New Haven: Yale University Press; London: Geoffrey Cumberlege: Oxford University Press. 1953. Pp. viii+353. 32*s.* 6*d.*

Readers of Dr Whiting's earlier work may well be puzzled and perhaps disappointed by the present book written in collaboration with Dr Child. It is an extremely ambitious cross-cultural study which examines child-training practices in seventy-five societies, relating the resultant personality types with various aspects of social organization. The method of approach is psychoanalytic, which has much to recommend it, provided those who employ it recognize that other branches of social science have something to add to the understanding of society which can be gained by psychological techniques. But the effective use of psychoanalytic concepts surely necessitates, first and foremost, a large amount of accurate information about the individuals and groups studied, and the ethnographic material about these seventy-five societies varies enormously in quantity and quality. There is, for instance, a great deal about the Navaho and Zuni, while the material on the Abipone was published in 1822 when modern ethnographic methods were non-existent, and the material on the Lapps consists of Turi's *Book of Lappland*, which, while fascinating reading, is hardly a book of anthropological significance, and Scheffer's *The History of Lapland* published in 1704. Moreover, even where the material is modern and reliable, the difference in approach and interest of various anthropologists makes it a risky procedure to compare their findings on topics which they may have regarded variously. We would, for instance, discover little about the metabolic processes of the human body by studying reports by a psychoanalyst on an Australian aboriginee, and by an Ear, Nose and Throat Specialist on an Eskimo.

But there is a further difficulty about the present study. The authors' conclusions are very largely reached on the basis of assessments by three judges (a logician and two psychologists) who have read the ethnographic material. These persons have been asked to 'evaluate the general effect on the children in a given society of all types of punishment and reward used in weaning, as he can judge it from the entire context of description of child training procedures in that society'.

Needless to say, the method is not quite so fantastic as this isolated quotation would suggest. A certain degree of statistical sophistication has gone into the correlation and evaluation of these judgements, while the judgements themselves are broken down into a large number of more specific questions. But no amount of ingenuity can quite conceal the unreality of the whole procedure. We know little enough about what happens in a single society for which the most complete first-hand evidence exists, and it is surely both premature and misleading to start system building at this stage of our ignorance.

However, notwithstanding the use of methods which cannot be allowed to go unquestioned, there is much which is stimulating and interesting in this book. The authors are men of sensitivity and wide experience who, however slender their grounds, have penetrating and challenging things to say on the general questions of culture-personality studies. They conclude their book by calling for more and fuller studies of society, and if they were not prepared to wait for these, they have at least illustrated our need for them. ADAM CURLE

Phantasy in Childhood. By AUDREY DAVIDSON and JUDITH FAY. London: Routledge and Kegan Paul Ltd. 1952. Pp. viii+188. Price 18*s.* net.

As the cover states, this work is for a psychoanalytically oriented public. The authors follow the Kleinian school. They have had a personal analysis, and, between them, experience in a children's club, child guidance clinic, hospital, nursery school, residential nursery for infants. And they worked with homeless children and with individual neurotic and psychotic children.

They show the kind of unconscious phantasies which operate at the various Freudian levels of development, and, during middle childhood, how these phantasies become modified by the experience of the actual world in which the child lives. In the last chapter, 'Living through of phantasies', an account is given of the working out of some phantasies in the life of a single child up to about three years of age.

The style is easy and flowing, and general statements are freely illustrated from particular children who are introduced by name straight into the text when it is appropriate. This makes for continuity of reading, but if anyone suddenly wonders, for example, which child is 'Charlie', then he will have some difficulty in finding 'Charlie' in the previous pages since he is so embedded in the text. Unfortunately there is no Index.

If the reader likes this way of presentation, which is a clever dovetailing of the theoretical with the concrete, he should find the reading a kind of living experience. There is more in the book to be pondered over than there appears to be, just because the style is so easy and the language unpretentious. It gives many vivid glimpses into the phantasy life of the child, and shows among other things how helpful and at times important it might be for a child that an adult should respond to this inner life to help him to a new understanding of himself, which in turn would enable him to cope better with life. The book is not dogmatic. Sometimes interpretations are merely suggested, while other interpretations must be based on much material not actually set down in it. Part of the value of the book lies in its suggestiveness.

A.S.P.

Understanding Children's Play. By Ruth E. Hartley, Lawrence K. Frank and Robert M. Goldenson. London: Routledge and Kegan Paul. 1952. Pp. xvi + 372. 21*s*.

This book describes a project carried out on children of two to six years of age, mostly in nursery schools. Different play activities were investigated to find out how far one could understand children from their play and in what ways they could make use of different play situations to express and to discover themselves. The conditions were purposely left uncontrolled but adapted to fit into each school programme and therefore varied from school to school and teacher to teacher in the freedom allowed to the children.

Each material or situation is discussed in a separate chapter: dramatic play, blocks, water, clay, drawing and painting, music and movement. A description is given from records taken at the time of what the children do with each means of expression and what it seems to do for them. Some case histories are given to show the relation between the different kinds of play and personal problems, and some estimate is made of the therapeutic value of play for children with difficulties. It is pointed out that more varied opportunities of this kind could be given in the ordinary school system since this report shows that with courage and thought even water-play can be arranged. To those educationists who may think life should consist of hard work it may be remarked here that children often work hard at play but this task is more often self-chosen and self-imposed.

Too much weight perhaps has hitherto been given to intellectual, moral and physical disciplines throughout our school education, and too little has been done to satisfy the emotional needs of the child for his balanced development; too much emphasis on learning things and too little upon experiencing them.

Much attention has been drawn to the use of play as a method of teaching young children and as a diagnostic and therapeutic method in clinical work. We must thank the authors of this book for carefully examining the process of play itself and showing that it could be a valuable part of the school life of the ordinary child. It serves, of course, a similar function to play outside school but differs in effect, chiefly because it is performed in what one might call a sheltered situation. There are an Appendix of Suggestions, Notes to Chapters, a Bibliography and an Index. A.S.P.

Practical Child Psychotherapy. By Curt Boenheim. London: Staples Press. 1953. 2nd ed. Pp. 184. 15*s*.

This book was first published in Great Britain in 1938. As its subtitle states, it is primarily a guide for the general practitioner. It discusses under the headings of certain physical symptoms or disorders in children such as loss of appetite, enuresis, asthma, to what degree they may be brought on by emotional stresses in the family situation, to what extent they should be treated, in addition to any suitable physical remedies, by psychological methods. These may vary from the simplest kind which any good physician would probably use, to cases which must be sent to a professional child psychotherapist. The author has drawn on his sixteen years' experience as a child psychotherapist in Berlin. The majority of references in bibliography are to German texts, but there are also references to a number of English ones. A.S.P.

Early Phases of Personality Development: a Non-formative Study of Infant Behavior. By SYBILLE ESCALONA, MARY LEITCH and others.

Monographs of the Society for Research in Child Development, Inc. Volume XVII, Serial no. 54, no. 1. 1952. U.S.A.: Child Development Publications. 1953. Pp. vi+72. No price given.

This monograph describes research aims and methods applied by the authors, their collaborators and assistants in the study of 128 infants below seven months of age. No definite findings or conclusions are so far presented, but certain further researches to be undertaken by various members of the team are outlined. These studies are all to be based on the same quite extensive data.

The work was undertaken by a total team of seven psychiatrists and psychologists, all engaged in clinical work at the Menninger Clinic. A staff of five secretaries kept the records. The interweaving of clinical and research activities, based on empirical studies of infancy, should be productive of useful results.

The primary interest of the team was in personality development and the approach was in the first place psychoanalytic. The study is described as non-normative; but, far from being merely descriptive as might be expected, many techniques including quite extensive measurements have been incorporated into the study. The interests apparently range from physical and anthropological measurements, medical investigations, psychological test results, environmental studies, psychiatric syndromes, modes of normal infant behaviour functioning and their variability, emotional factors, to mother-child relationships, etc.

Methods used include interviews with mother and child in a specially and quite elaborately prepared centre, detailed observations of infant behaviour in a variety of situations, home visits, cinematograph records, tests of intelligence, physical examinations. The psychological tests used were the Cattell Infant Intelligence Test, and the Gesell Developmental Schedules. Special observational procedures were also undertaken: tissue resistance, leg resistance, strength of grip, etc. A stabilimeter was used to measure (by means of kymographic tracings and counter readings), the baby's movements in greater detail. Activity ratings, for example before and after feeding, were assessed. Two films were made of each baby to show feeding behaviour and other spontaneous behaviour. Running records and notes were also kept by additional observers.

The result of all this work, which has already occupied a number of years, is a very extensive mass of data that the authors expect will take several further years to organize and summarize. A form consisting of no less than 40 pages of headings has been prepared for the summarizing of the material in each case. The studies which it is proposed shall result from the data include: (1) exploration of infantile functioning in three body systems—the sensorium, the skin and the gastro-intestinal; (2) a survey to study whether behaviour syndromes can be delineated; (3) maternal attitudes and determinants of specific attitudes in infants; (4) sleep patterns; (5) maternal behaviour in feeding infants; (6) psychological test findings and 'adjustment status' in infancy; (7) a study of tact; (8) traditions about infants and infant management; (9) hand and arm movements and postures in infants; (10) the emergence of voluntary, purposeful action.

This piece of work as described should prove of interest to a wide audience, as it attempts to cover or touch on an enormous number of facts and topics of medical, psychological and psychoanalytic interest. This is perhaps also its weakness, that it has attempted to do so much that it may prove difficult to crystallize out information that is new or significant. Starting as a 'non-normative study', it appears that norms of many kinds have yet been introduced as the work proceeded, and the resulting data should provide a valuable core from which useful and exact information could be extracted. It will be of great interest to note what use is made of this material, both normative and descriptive, in the contemplated researches. RUTH GRIFFITHS

The Abilities of Babies. By RUTH GRIFFITHS. London: University of London Press. 1954. Pp. x+229. 20*s*.

In this book the author has tackled the difficult task of evolving a scale for measuring the mental development of babies up to two years of age. It is written in three parts. The first, based on careful observation of babies, and full consideration of existing tests, points to the possibility of a new scale with a new technique for assessing the abilities of this infant group. The second part details the seeking,

searching and researching necessary in order to allow of the building and presentation of such a scale. The third demonstrates the method of administration of the new scale which has been founded on aspects of development that appear significant for mental growth. In its published form the scale consists of five subscales lettered *A* to *E* and all very nearly equal in difficulty. Each subscale covers one vital aspect of infant development namely: *A*, locomotor development; *B*, personal-social adjustment; *C*, hearing and speech; *D*, eye and hand co-ordination; and *E*, performance.

From a summation of the scores from these five subscales Dr Griffiths gives a means of obtaining a G.Q.—General Intelligence Quotient. Apart from this G.Q. it is also possible to calculate *QA*, *QB*, *QC*, *QD* and *QE*, each representing respectively a quotient indicative of the child's ability in each of the five subscales. Since these five scales are very nearly equal in difficulty it is possible to show the total mental level attained by a subject at the time of testing. This has been done by the profile method, which from the diagnostic point of view should prove most valuable in that it depicts general or specific disabilities or the presence of exceptional ability in one or more of the basic developmental processes.

The inclusion in the book of developmental profiles of particular cases, and of excellent photographs of the responses of babies to different situations, adds to the clarity of the exposition. There is an excellent bibliography. Methods of scoring the tests and assessing results are assembled with admirable economy and clarity in Appendix I.

The Abilities of Babies will be of interest and of value to all concerned with the health and well-being of children, and particularly to the psychiatrist and the psychologist working in the field of child psychology. It offers a new approach to assessment and to diagnosis. It suggests many new avenues for further research. This scholarly work commands respect for its clear exposition of each step in procedure, its integrity of observation and report, and its wealth of detail which illumines the text without tedium. Young research workers will find research procedures fully mapped out.

Dr Griffiths intends to 'continue the work on the new scale beyond the second year and this will then make differential diagnosis as outlined in this book available for testing children over two years of age'. We look forward to the completion of this further research. Such a scale carried to years five or seven would prove invaluable, particularly in the field of the handicapped child. MARGARET MARTIN

Childhood Problems and the Teacher. By C. Bühler, F. Smitter and S. Richardson. Routledge and Kegan Paul. 1953. Pp. xii+262. 25*s.*

Educating the Subnormal Child. By F. Lloyd. London: Methuen. 1953. Pp. vii+148. 11*s.* 6*d.*

The Education of Exceptional Children. A. O. Heck. 2nd ed. New York, Toronto and London: McGraw-Hill Publications Inc. 1953. Pp. xiv+513. $6.00.

These three books on child psychology are concerned with children who are deviants from the normal.

The first of these, as its title suggests, is written specifically for the teacher and attempts to show how everyday problems in the school may be solved by the teacher. The book is the work of three authors who are experts in the field, a fourth writer contributing a chapter on remedial teaching. Children's drawings illustrate the text and their interpretations give excellent insight into the difficulties and problems of their respective creators.

The majority of the disturbances which arise in school or in class need to be treated only on the surface, but others may require deeper psychological study. Symptoms of such disturbances are discussed and it is suggested that the teacher who has sufficient psychological knowledge and has made a study of personality problems may be able to deal with some of these. Others require the more expert advice of psychologist and psychiatrist.

Part I deals with the psychological approach to children's problems. Certain sections, which consider basic concepts, appear to be too summarized for easy reading, and presuppose some knowledge of psychology. Part II, concerned with what the teacher can do, forms the main part of the book and contains discussions of case histories. Part III is an assessment of the psychologist's collaboration with the school and is concerned with diagnosis and psychotherapy. It also contains a chapter which serves as an introduction to projective techniques.

The book draws attention to problems met with at school and should be of interest to intelligent parents and teachers. It presupposes, however, that teachers have a fairly extensive knowledge of clinical work, have access to the child's home, and that parents always co-operate.

The author of the book on educating the subnormal child is in charge of a school for subnormal children,

and the content of the book is an attempt to convey to the reader her attitude to the children under her care. Individual understanding is the main theme and examples are given of children who show all-round improvement in an environment in which they feel secure.

This is not so much a book for the professional psychologist as for the special school teacher, whom it will stimulate to further endeavour.

The volume from the hands of Prof. Heck has brought up to date an account of the education of exceptional children first published in 1940. One chapter has been wholly rewritten and sections and paragraphs in others have been revised. Although the setting is in the U.S.A. the contents of the book should be of value to all those concerned with exceptional children. The contents are divided into four parts, the first three considering in turn the education of the socially handicapped, the physically handicapped and the mentally exceptional. The term exceptional not only applies to all those who are handicapped, but also to those who are gifted, either generally or in one specific direction. The fourth part discusses problems of administration. Photographs are included which illustrate new equipment and new procedures, and for those interested in any type of children who require special education the book should prove stimulating. It is sufficiently non-technical to appeal to the non-specialist, although it was written for use in college and university courses. MARY COLLINS

Family Environment. By JOHN D. NISBET. London: The Eugenics Society and Cassell and Co. 1953. Pp. 51. 3*s.* 6*d.*

A negative correlation between family size and intelligence has been reported by many writers, even after environmental factors such as occupation have been eliminated. In this study the author gives a carefully reasoned account of the many problems involved in assessing the relationship; he gives an extensive survey of the literature on the subject and also discusses the verbal factor in mental development. He then puts forward the hypothesis that a large family is in itself deleterious to the mental development of a child, particularly in so far as it affects verbal development; in support of this hypothesis he presents the results of a survey carried out on Aberdeen school-children. These results are certainly of interest, but his conclusions from them are not altogether convincing owing to a major fault in an otherwise carefully planned survey. The correlation between family size and certain test results which he has calculated are obtained from all Aberdeen schoolchildren in certain age groups and no account has been taken of varying social class or occupation of parents, which is known to be related to family size. Unfortunately these factors could very well account for his results, so that his conclusions are almost entirely vitiated. M. A. CREASY

Social Implications of the 1947 Scottish Mental Survey. London: University of London Press for Scottish Council for Research in Education. Pp. xxiii+356. 10*s.* 6*d.*

In the second volume on the results of the 1947 Mental Survey of Scottish 11-year-olds Mr James Maxwell makes a careful and thorough analysis of the sociological data collected about a random sample of 7380 children from the total group of 75,211, namely those born on the first three days of each month of 1936. This analysis shows that there is a steady decrease in average intelligence test score, height, weight, frequency of migration from district to district and age of mother as one ranges from professional and large employers through salaried employees, small employers, non-manual wage-earners, skilled and semi-skilled workers to unskilled manual workers; and at the same time a steady increase in family size and overcrowding. *Within each occupational class*, however, average test score shows a definite downward trend as family size increases. No marked differences were found between the average test scores of children living in 'Cities', 'Large Towns', 'Small Towns' and 'Other Areas'.

Mr Maxwell frankly admits that the Survey 'raises more problems than it solves'. It offers, for instance, no further light on the problem of the relative contributions of heredity and environment or of the different factors within the environment to the better (on the average) performance of the 'upper class' child from a good home on an intelligence test, but it is 'fairly definite' in its conclusion that 'intelligence, as measured by the test, does vary with environmental circumstances to a considerable degree'.

Included in the volume is an analysis of the test scores of 947 twins from the *total* survey group. This confirms the observation of other investigators that twins are on average four or five points of I.Q. below non-twins, but throws no further light on the reasons for this difference. ALEXANDER LAING

Education in England. By W. P. Alexander. London: Newnes Educational Publishing Co. Ltd. 1954. Pp. xi+147. 12*s.* 6*d.*

The subtitle of this book is 'The National System—How it works'. This gives the aim which Dr Alexander had before him. In the foreword he comments on the difficulties encountered by visitors from abroad in trying to grasp how the English educational system is organized. The book attempts to meet these difficulties, and no doubt foreign visitors—and our own students—will be grateful for this attempt to fill the gap in our library shelves. The absence of any previous volume of this kind is perhaps an indication of the difficulty of the task, and it would be easy for educationists to criticize the book for what it omits—with such a complex subject this is inevitable in a work of this size.

The reviewer was surprised at the absence of any diagrams and 'flow-charts' which have the advantage of presenting complexity succinctly, and which (particularly for the foreigner) are sometimes less subject to misinterpretation than the many paragraphs of print necessary to outline the same ideas verbally. Dr Alexander is a distinguished administrator, and one feels that the book presents the machinery of the system without sufficiently indicating the parts of the machine that work smoothly and those that creak and groan a little. The foreigner may well feel frustrated over some aspects—for example, special schools for the educationally subnormal are given a subsection, yet the criteria adopted for the ascertainment of the children are not referred to, nor is any indication given of the numbers involved. The Ministry pamphlet covering this point is mentioned in the text, but its full title is not given, nor is it included in the appendix of references. The absence of an index to the book is particularly to be deplored, even though the table of contents is given in detail.

These criticisms, however, do not affect the judgement that the book as a whole is one which fills a real need of the student and the visitor from overseas. STEPHEN WISEMAN

Psychology Applied to Human Affairs. By J. Stanley Gray. New York, Toronto and London: McGraw-Hill Publications Inc. Second edition. 1954. Pp. viii+646. 42*s.* 6*d.*

This second edition of the 1946 publication again contains seven chapters by the author-editor, together with chapters from ten other contributors. It is a text-book of applied psychology, designed to follow a short introductory course of general psychology. The following fields are covered—college life, child development, educational psychology, vocational guidance, personality adjustments, speech correction, mental illness, clinical psychology, effects of diet and drugs, crime and crime detection, psychology in industry (four chapters, including a new one on water determination), marketing and advertising, military psychology, music and art, public opinion and propaganda.

The general impression of this slightly more up-to-date edition is that its claim to be a bird's-eye-view only, is fully justified. The author points out that it needs a teacher 'to interpret facts and answer questions'. Certainly much of the material is over-concise and could be misleading. Apart from some useful hints to students of the mental hygiene sort, a course in applied psychology would still be more usefully based on existing texts of a less comprehensive kind.

If one were to judge from this book, almost no applications of psychology to human affairs have been made in Britain. Only a very few articles mostly published in the twenties, are mentioned.

It is a pity that a costly book of this length should be so unrewarding. FLORENCE MACNEILL

Transactions of the Conference on Morale—And the Prevention and Control of Panic. New York: 1954. Pp. vi+75. No price given.

This impressively titled report was no less impressively sponsored—by the New York Academy of Medicine and the Josiah Macy Jr. Foundation. The 75 pages of typescript contents hardly bear such a weight. There are seven papers: J. A. M. Meerloo, 'People's Reaction to Danger'; G. M. Gilbert, 'Social Causes contributing to Panic'; H. Brucker, 'The Press in relation to Morale and Panic'; R. Saudek, 'Radio in relation to Morale and Panic'; G. S. Stevenson, 'The Voluntary and official agencies Role in the Promotion of Morale and the Prevention of Panic'; J. S. Tyhurst, 'Research on Reaction to Catastrophe'; and L. Alexander, 'Morale and Leadership'. In a book planned for publication and serious criticism this combination of authors and titles would have been promising.

Unfortunately we are given only the verbatim report of short speeches with these titles, given to what must have been a very mixed audience. At least, the speakers, with few exceptions, took no risks and preferred the colloquialism to the precise statement, the anecdote to the reference to research, and the extended cliché to the novel insight. The information we have gained from MacCurdy's 'Structure of Morale', the wartime collection of papers on 'Civilian Morale' edited by Goodwin Watson, and the vast series of attitude surveys carried out by the Information and Education Department of the U.S. War Department, is neither usefully summarized nor valuably extended. One is indeed struck by the combination of statements emphasizing the importance of research into problems of morale and panic with an apparent ignorance of even the best known work carried out in this area. At least one of the speakers, however, was concerned solely with the problem of psychiatric 'first aid' in a situation following a catastrophe, and Dr Meerloo in an admirable paper gives a series of hints for the mass treatment of acute anxiety states.

In general, one's feelings about such publications are mixed. Conferences attract publicity, and a glance inside the bright red covers of this report may have directed the attention of some foundation executive to the research needs of the social sciences. But the gulf between the spoken and the written word is too great, at least on this occasion, for one's reading to be very enjoyable or fruitful. Perhaps the predominating impression is best conveyed by Dr Iago Goldsten in his concluding remarks, when he says 'I should here give a résumé, but I am afraid I shall fail you on that score. The word résumé, as you know, means to resume. To resume means to begin all over again and on this occasion to begin all over again would indeed be terrible.' J. F. MORRIS

The Nature of Choice in Casework Process. By ANITA J. FAATZ. North Carolina University Press; London: Geoffrey Cumberlege. 1953. Pp. xii+141. Price 24*s*.

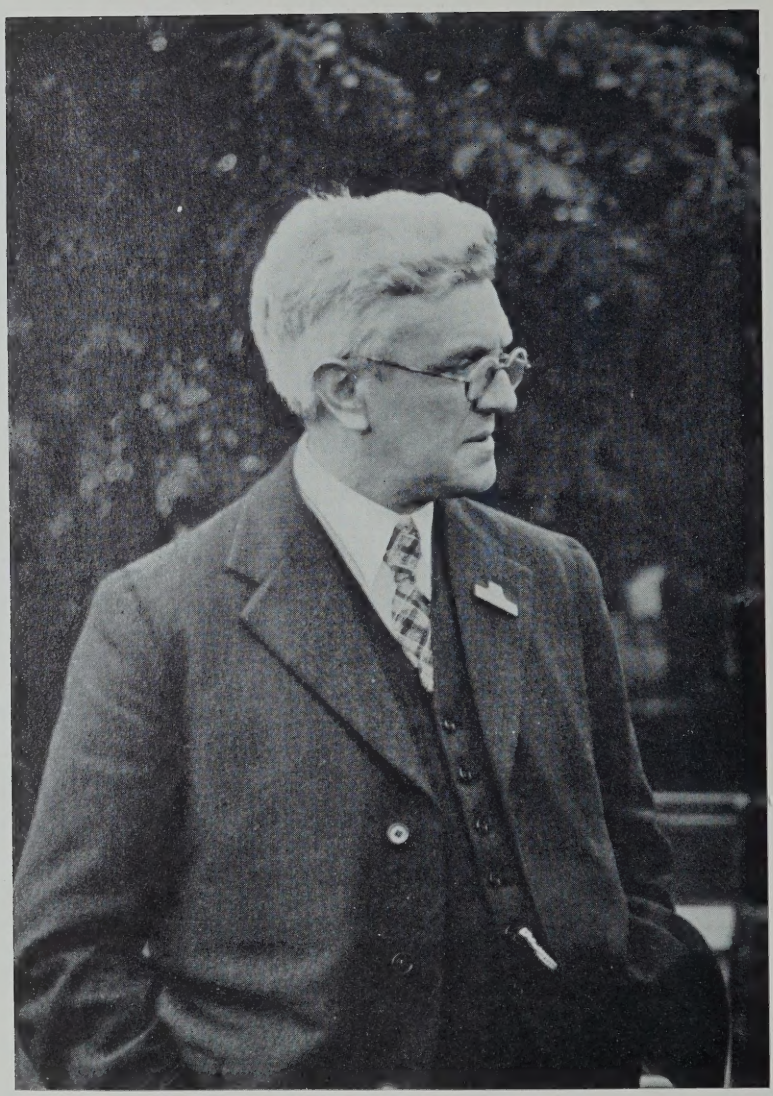
This book discusses the freedom of action and behaviour of the client of the social worker from the point of view represented by the author. This point of view found its expression in the will psychology of Otto Rank, who has had such great influence in various directions in the U.S.A. but is less well known in Britain.

The discussion is preceded by a historical account showing how the concept of the freedom of the individual first arose in connexion with social work and has been stressed as important from time to time from 1880 onwards, and has changed in meaning.

The main part of the book shows what is this freedom (nature of choice) a client should enjoy. It is assumed that anyone who comes to a social agency for help—no matter how simple—has projected his own inner difficulties upon the outer world. Those difficulties with which the individual is striving are the concern of the social worker, within the framework of the practical problem brought to him.

In order to explain how the social worker can help his client with these difficulties and yet leave him free each time to make his own choice, it is necessary to explain the whole 'functional helping process', which involves explaining the caseworker and his development in experience and depth of understanding and the relationship between client and worker. This is done step by step until we arrive at something very like the psychoanalytic process very ably described, partly in Rankian terms and partly in those suitable to the context of social work and the author's mode of thought. The determinism of Freud is repudiated and the freedom and responsibility of the individual are postulated. At each stage he is free to choose his actions and he is responsible for his choice. The social worker 'stands by', nautically speaking. In this context the desirable behaviour is to take (to will) the choice which leads towards the growth or maturity of the self. Nevertheless, the crisis in the 'helping process' comes when the client accepts the inevitable and sets out to achieve a new relationship to it. Although this is described in a rational way the process itself is not primarily a rational or intellectual one. The 'will' is described as 'the expression of the creative self'.

The account is very abstract, and the American technical or semi-technical language is at times slightly repellent to the English ear, probably because we are inured to our own jargon. The author has had much experience and has thought deeply before writing this book. It sets out a philosophy and a goal for social workers on the therapeutic side of their profession, and would also be of value to anyone engaged in dealing with other people in an advisory or helpful capacity, and who wants to understand what may lie below the superficial level of the immediate situation. A.S.P.



STANLEY J. F. PHILPOTT, 1888–1952